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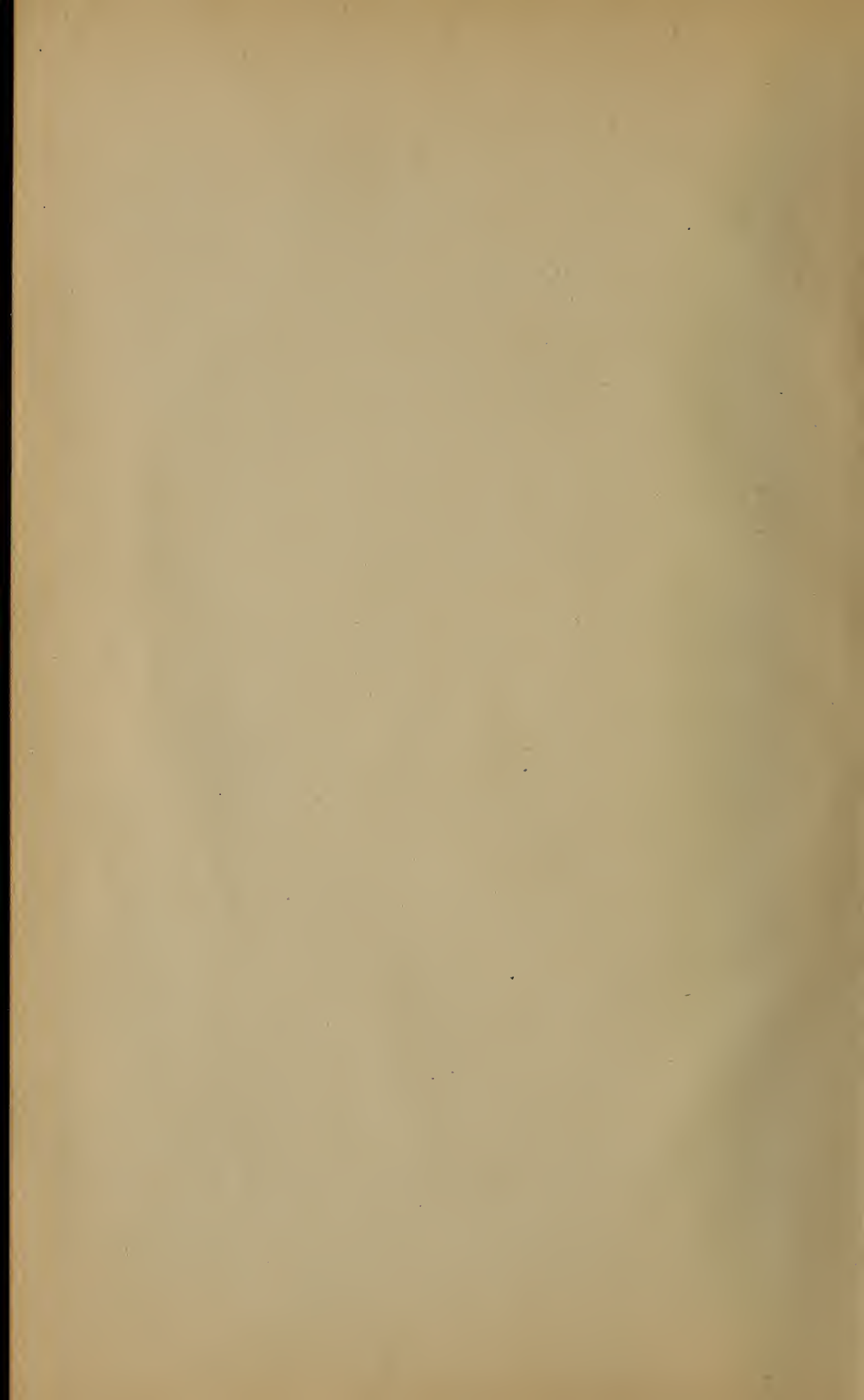
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YELLOW FEVER:

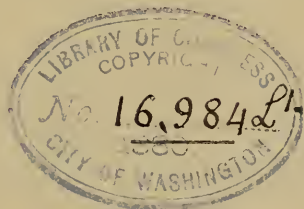
ITS SHIP ORIGIN

AND

PREVENTION.

✓  
BY ROBERT B. S. HARGIS, M. D.,

PENSACOLA, FLORIDA.



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## INTRODUCTION.

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The dangerous and irrational proceedings, constantly advocated, in relation to the prevention of yellow fever, induce me to reprint three articles, which have recently appeared in medical journals, and which, if I may judge from kind letters received, have met with approval on the part of many of my professional brethren. Twenty-two years have so familiarized me with the idea, based on many facts, of the naval genesis of this disease, that I am much in the position of all who acquire positive knowledge on any question, and who become intolerant of error. A man who insists to-day that the earth is flat is simply regarded as an idiot, whereas Columbus, when leading that immortal expedition which was the forerunner of our new world life, and of all yellow fever epidemics, had great difficulty in convincing his adversaries and followers that he was not going to the edge of the earth's broad plane, thence to drop into unfathomable space.

I plead guilty to a charge which may be made against many who acquire firm convictions, viz., that I did not take time—which, in truth, I could ill afford—to publish in book form, such evidence as had converted me from being a believer in the land origin of the disease. Evidence of this character was brought together, last year, by Professor John Gamgee in his work on “Yellow Fever a Nautical Disease,” which was in the press when my views were first made known to him. A careful study of the question enabled him to define the geographical limits of the disease, its probable origin in the calm belts, the exemption of all other seas except the tropical Atlantic, and that it never

had penetrated continents, but had confined its ravages to seaports and places in direct communication with seaports. He was as impatient, as I was, at the insinuations that yellow fever was a disease of the American continent, as expressed by a German author, and failed to conceive how any one could imagine that it developed spontaneously in New Orleans or any of the Antilles.

From the publication of my first paper, in the New Orleans *Medical News and Hospital Gazette*, in 1858, occasional newspaper articles and discussions had led some physicians to understand my views; but the necessity for wide and repeated statement of the simplest truth is indicated by that admirably edited work, Parkes' *Practical Hygiene*. Both Professor Parkes and Doctor De Chaumont, who are responsible for this work, have long been connected with an army medical school; and it is matter of history how seriously the British soldiers and sailors have, until within recent years, been afflicted with yellow fever in the West Indies. Turning to the chapter on the prevention of some important and common diseases, we find that "the question of the origin of yellow fever is one which *cannot be considered in this volume*, and at present no preventive rules of importance can be drawn from the discussion."

Is it possible to acknowledge impotence in stronger language, when a studious review, of the cause and development of yellow fever, could scarcely lead to any other conclusion, than that which Copland found himself conquered by, viz., that, after all, no opinion could be sustained with more irrefragable evidence than that yellow fever originated in foul ships?

To those who know what the inhalation of nitrous acid fumes implies, and who have experience of the slender touch that kicks the beam, in the balance between life and death, in yellow fever, the following sentence must sound

absolutely inexplicable : “ In all buildings *where sick are, or where yellow fever prevails*, there should be constant fumigation with nitrous acid, which *seems* to be, as far as we know, the best disinfectant for this disease.”

The only possible and sufficient purifier is an unlimited supply of wholesome air, and when adequate ventilation, without the poisonous contamination of adjoining rooms, houses, or streets, may be secured, the irritating, killing, and worse than useless, vapors, so abundantly prescribed, will fall into disuse. Bearing on the influence of pure or impure air as the cause of yellow fever, I desire to quote the words of an excellent observer, for whom I entertain a great respect, and who was commissioned, in consequence of a suggestion of mine, to investigate yellow fever. In the second volume of the public health reports and papers, published by the American Public Health Association, Dr. Harvey E. Brown, Surgeon United States Army, has a paper on Yellow Fever in the Dry Tortugas. In concluding it he says :

“ I desire to offer a few remarks concerning the question of the transfer or isolation of population from infected localities. This, to be sure, is no longer an open question. Its advisability has been proved in so many instances, that it is only on the ground of accumulating testimony on its advantage that it is worth while to mention it.

“ It is a curious fact in the history of yellow fever (and, perhaps, of other infectious diseases) that a community already infected may be moved from the locality where the disease first appeared ; and that in its new and healthy location, no new cases will occur, except among those who were passing through the period of incubation at the time of such removal. In July, 1867, yellow fever was carried by a sick man from the town of Indianola, Texas, to the permanent military camp, distant a mile from town. Twenty-two cases and twelve deaths occurred, after which the command was moved to Green Lake, a healthy country locality some

twenty miles distant. But two cases occurred after the removal, the remainder of the command keeping perfectly well, while a few soldiers, who had been left behind at the old post, all had the disease.

"In the epidemic of 1873, at Fort Jefferson, the troops were removed to Loggerhead, Ky., on the 28th August, a week after the disease had become epidemic. After removal, one man was taken at Loggerhead on the 29th, one on the 31st, and two on the 2d of September, all of whom were promptly returned to the post, after which the disease ceased at Loggerhead, and the command remained perfectly healthy. When the troops moved, several men remained behind at the fort; all but one of these took the disease, and that one was the ordnance sergeant, who lived in a distant part of the work, and held no communication with the other parts of the fort.

"The removal of the sick to a distance, leaving those who have not been attacked behind, was shown to be of no value in the epidemic of 1870, on Governor's Island, New York harbor—cases of the disease continuing to occur for a month after the sick had been so removed. This absence of disposition to spread (as we may call it) has always been used as a strong point in favor of the non-infectious nature of the disease, but a little consideration will show the fallacious character of the argument. The conditions in the two localities are entirely different. When the fever breaks out among troops or permanent barracks, its generating principle quickly infects the walls, wood work, and furniture of the quarters. Moreover, within the walls of a fort or in barracks, *there is always a certain amount of residual air*, which the ordinary wind-currents are not sufficient to entirely remove. This speedily becomes charged with the poison, and thus resembles *that contained in the infected hold of a ship, producing the disease in all susceptible persons who respire it*. Let, however, the population of such an infect-



ed locality be removed into tents in an open country, and all these deleterious conditions disappear. The tents should be so arranged as to afford a frequent and complete renewal of the atmosphere; if they become infected, they and their contents should be immediately destroyed.

"We may lay it down as an established fact that the cessation of yellow fever, and its inability to spread after removal of the well people from a locality where it exists, so far from its being any argument against its infectiousness, is really one in favor of it, for this reason: where *the sick are moved instead of the well*, as at Governor's Island, in 1870, the epidemic continues, all the conditions for its propagation being still present."

Would that observers had always been as precise and reporters as logical as Dr. Harvey E. Brown. We should then not have to criticize the excellent compilations which are mixing "chaff with the wheat" offered to medical students and sanitary officers. Can anything be more preposterous than the plan adopted in the British Navy, endorsed by Parkes, who says, "as a matter of precaution, the present plan of three or five years' Mediterranean service before passing to the West Indies seems desirable."

In his New Orleans lectures, Mr. Gamgee showed that the enclosed seas, though the hottest, such as the Mediterranean and the Red sea, have always been exempt from yellow fever. It only penetrated, by importation, to Minorca, Leghorn, Malta and Egypt early this century, carried by contaminated vessels, infected directly or indirectly in the West Indian Archipelago. It is this scrutiny of history which arms us with facts, such as no authority has been able to adduce in relation to the spontaneous origin of the yellow fever on land. Mr. Gamgee has ransacked every source of information, as I have done for over a quarter of a century, and, apart the interesting manifestations of Oroya Fever in the Andes, nothing which could in any way be

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confounded with Yellow Fever has been traced to specific land influences. History, accurate knowledge of genuine endemics, the geographical distribution of disease and the overwhelming evidence of outbreaks of yellow fever in ships without contact with infected lands, may be relied on to confront and confound the "opinions" of those Port Physicians who indulge in catastrophic etiology. Noah Webster—a great man, but a bad pathologist—is really their luminary, and no one since his day has equalled him in marshalling all facts, *but* those right ones, on which, justly interpreted, now is woven an indelible generalization—that Yellow Fever is the Ship Fever of the Tropical Atlantic.

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## THE SHIP ORIGIN OF YELLOW FEVER.\*

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America may well be proud of the attitude assumed, and successfully maintained, by its medical men, on the great question of Public Hygiene. State Boards of Health, the American Public Health Association, and last, but not least, the National Board of Health, represent the vigorous efforts of practical Sanitarians for the permanent advancement of the country's welfare. A boon beyond all price to the New World is to result from efforts aimed mainly at the prevention of Yellow Fever. Whatever conflicts of opinion or errors in practice may retard the work, I am confident, as I said years since, that under an adequate central administration by a Medical Board, the plague of American commerce, more especially of Atlantic sea ports, can be stayed and extinguished for all future time from the bloody annals of controllable epidemics.

One year's experience of the operations of the National Board of Health satisfies me that the disease is being attacked locally on land as a contagion, whatever the precise method of transmission may be, and that special centres for the isolation and purification of shipping are to liberate commerce of unnecessary inter-State obstructions. The practical difficulties encountered will suggest the details of a preventive system. The land will be cleared of the malarial and its threatened invasion in shipping will be met rationally. If I know anything in medicine—if there be any medical truth which I have grasped by searching methods of careful study—it is that, year by year, a fresh crop

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\*Reprinted from Gaillard's Medical Journal for June, 1880.



of poison springs from foul vessels sailing in the tropical Atlantic, and from such infected ships, or their contents, and others infected directly or indirectly by them, the country can be protected without onerous restrictions on commerce.

As physicians we must never forget that prosperity is the surest key to national vigor and individual health. Obnoxious quarantines and schemes of non-intercourse imply an acknowledgement of our impotence. Happily we are far from being driven to this extremity. The meaningless term, quarantine, should be made obsolete. It is odious to all and its use excites more enmity than the most tedious safeguards we have to impose. Professor John Gamgee, whose work on "Yellow Fever, a Nautical Disease," should be studied by every medical man, has suggested to me the expression "Sanistation," instead of "Quarantine Station." The word sanistation refers to the practical preventive or purifying measure to be applied on land or ships. Why not speak of "Ship Island Sanistation" instead of "Ship Island Quarantine"?

An unexpected incident has induced me to direct the attention of my professional brethren to a correspondence which has appeared in the *Pensacola* and other papers. In reply to a circular issued by Dr. J. C. LeHardy, of Savannah, relating to proposed Congressional action affecting the National Board of Health, whilst strongly advocating that the powers of the Board should be increased rather than curtailed, I remarked as follows:

"The National Board of Health has been unfortunate in favoring the view that Yellow Fever is endemic in Cuba. The report of the Havana Commission must, before another decade is past, meet, as it deserves, with the strongest condemnation. Such is my opinion on this point. It is difficult enough to get the Spanish Government to do its duty, towards this country, by purifying the Pearl of the Antilles,

and how can we hope for their energetic action when an American Physician, clothed with all the prestige of authority, assures the world that Yellow Fever in Havana is inevitable?

“In this most healthy region, (Pensacola, Fla.,) singularly free from contagious maladies, where climatic seasonal conditions only rarely favor the propagation of imported yellow fever, and where the winter hibernation of the malady has been proved impossible over a period, to my knowledge, of 30 years, how can we throw open our Port to commerce, in summer and autumn, when a single ship from Cuba having one case of the disease on board, will close all inland communication?

“It is, I confess, with absolute impatience that I learn of the continued advocacy of the local origin of yellow fever, either on this Continent or in the West Indian Islands. The blunders of the Profession are still in striking contrast to the common sense of the people, on the one vital question of the communicability of this disease. The eloquent teachings of shot-guns quarantine—detestable as every form of real or apparant inhumanity must be—may yet drive home the substantial truths, in relation to the transmissibility of the malady by Railroads and Refugees.”

This provoked an answer from Dr. Chaillé, which tends to remove any doubt as to the tendency of the Report, which so far as I am aware has not been discussed in the Medical Journals. That such discussion is urgently called for, I trust to prove by comments intended to avert mischief if they fail to elicit truth.

NEW ORLEANS, *March 19, 1880.*

EDITOR OF THE PENSACOLA ADVANCE :

DEAR SIR : My attention has been called, this day, to the letter of Dr. Hargis on yellow fever, published in your issue of March 10, 1880, and I respectfully submit that in opening your colums to a denunciatory attack, common justice requires that equal publicity should be given to the defence.

Dr. Hargis, whom I know, and have had reason to regard with friendship and respect, states: "The National Board of Health has been unfortunate in favoring the view that yellow fever is endemic in Cuba. The report of the Havana commission must, before another decade, meet, *as it deserves, the strongest condemnation.* Such is my opinion on this point."

Dr. Hargis would confer a favor by stating when and where the National Board of Health "unfortunately favored" any such view. This board did instruct its Havana committee to investigate "the *so-called* endemicity of yellow fever in Cuba." I am not aware that it has ever said or done more than this to unfortunately favor a view, in opposition to which Dr. Hargis will find it difficult to cite one single Cuban authority, of any weight in medicine. In Cuba yellow fever is very certainly a "so-called endemic."

While this is the non-committal position of the National Board of Health, the Havana yellow fever commission calls attention to the indefiniteness of the signification of the word "endemic," and then states that if the word is accepted as signifying an habitual annual prevalence of a disease, and the existence of causes favoring its propagation, *then* the facts prove that yellow fever is endemic in certain localities in Cuba. The facts, on which this conclusion is based, are stated, so that no man can be led astray either by the indefiniteness of the word endemic, or by the theories and fancies of the commission, which, under the circumstances, will await with great equanimity the expiration of Dr. Hargis' fatal decade. So much for "this one point."

Dr. H. also writes: "It is difficult enough to get the Spanish Government to do its duty towards this country by purifying the Pearl of the Antilles, and how can we hope for this energetic action when an American physician, clothed with all the prestige of authority, assures the world that yellow fever in Havana is inevitable?" I, Mr. Editor, am this unfortunate unrepentant "American physician." Here, again, as everywhere, the facts were first given, in order that "the world" might correctly judge the validity of the conclusion. Those facts are, briefly, that since 1761 yellow fever has annually prevailed in Havana; that all the favoring conditions during the past one hundred and twenty-eight years persist; that there were no evidence that these conditions would be arrested, and that as long as these conditions persisted it was inevitable that the effect would persist. The Havana commission is well assured of the correctness of the premises of this syllogism, and submits the conclusion with great confidence to the test of logic.

In fine it seems that Dr. Hargis is an earnest advocate *for the origin of yellow fever on ships*—a view which the Havana yellow fever commission earnestly condemns, and here, I imagine, is the great cause



of his offence against, and condemnation of, the commission. But the commission was careful to state the facts on which its opinion was founded, and feels very confident that however false this may be, all just and generous men will view with liberal charity any such conclusion when preceded by and accompanied with the very facts from which it was deduced.

In conclusion, Mr. Editor, permit me to make a prophecy about that "next decade" with which Dr. Hargis so unmercifully threatens the Havana yellow fever commission. This prophecy is, that at its expiration many more doctors than at present (though they are numerous) will think it as idle a waste of words to discuss the *origin* of yellow fever as to discuss the *origin* of a cat or of a dog. The great practical question is with yellow fever (as with all vegetables, animals, and epidemic diseases), how is it *propagated*? For my own part, I have even less hope of determining the *origin* of yellow fever than the *origin* of small-pox, scarlatina, typhoid fever, cholera, trichina, tape worm, etc. Now, if Dr. Hargis will inform us how the causes of any of these *originate*, while handling with so much confidence the origin of yellow fever, he will confer an inestimable favor on science and mankind. In order that the correctness of my statements may be tested, I forward you a copy of the report of the Havana yellow fever commission.

Yours, very truly,

STANFORD E. CHAILLÉ, M. D.,

*President of Havana Yellow Fever Commission*

*of the U. S. National Board of Health.*

To which I replied as follows :

EDITOR PENSACOLA ADVANCE :

SIR: Grateful am I for the publication this day of my friend Dr. Chaillé's remarks on my letter which appeared on the 10th inst. I have known Dr. Chaillé for many years. It is the privilege and duty of professional men to discuss scientific questions, with ardor and force, based on intelligent conviction, whilst avoiding personalities. You, sir, have enabled Dr. Chaillé to define his position, and to stand in self-defense in the most manly attitude, not sheltered behind the titles of the Havana Commission and the National Board of Health. I accept his challenge only for truth's victory and the demolition of specious argument or theory based on false premises.

Dr. Chaillé holds himself responsible for "a concise preliminary report, stating the general results accomplished" by the Havana commission, so far, at all events, as the question in dispute, and which refer to the endemicity of yellow fever in Cuba and the origin of the disease in ships and harbors.

Whatever mystery there might have been as to his interpretation of the word endemic it is dispelled. It is used by the Havana yellow fever commission to signify the "habitual prevalence of a disease and the existence of causes favoring its propagation." The "facts are, briefly, that since 1761 the yellow fever has annually prevailed in Havana; that all the favoring conditions during the past one hundred and twenty-eight years persist; that there were no evidences that these conditions would be arrested, and that as long as these conditions persisted it was inevitable that the effects would persist."

Words in diplomacy may be used to conceal man's thoughts; the language of science should be precise. Webster, after Hoblyn, defines *endemia* or *endemic* as signifying "peculiar to a people or a nation, and *endemization* as equivalent to naturalization, to indicate an indigenous growth. We speak of *ague* as an endemic of the marshes. It is an *autochthonous* disease springing from the soil—from local conditions. Cholera is the endemic of Hindostan, and only known to us in America like yellow fever, as an imported plague, pestilence, or epidemic; and, at most, medical men have spoken of sporadic cases, to indicate isolated attacks, before or after epidemics, or as casual accidents. When the Havana commission, in its report, speaks of small-pox as endemic in Havana, it confounds yellow fever with the purely contagious maladies, and classes it where it cannot for one moment be placed by an enlightened pathologist. That Dr. Chaillé commits this serious error is shown by the important conclusion, that "it is as idle a waste of words to discuss the origin of yellow fever as to discuss the origin of a cat or dog"—"for my own part," he adds: "I have even less hope of determining the origin of yellow fever than the origin of small-pox, scarlatina, typhoid fever, trichina, tape worm, etc."

The fact, sir, that yellow fever is totally unlike any of these diseases, that it is entirely specific and distinct, and, unlike any other known plague, invades land rather than springing and spreading from land, stamps it with peculiarities which enable us to determine its origin. There is no more interesting truth in pathology, nor one better established to those who are not blind or deaf to the clearest evidence, that the yellow fever poison, engendered and reproduced outside the human system, is the active cause of an infection and malignant pestilence which may travel under favorable conditions wherever man may go, even as far north as Quebec. The correct history of yellow fever places it indisputably amongst the localized diseases of the globe, and its habitat since the discovery of the new world, *and not before*, has been the tropical Atlantic. It has not originated in the Pacific, nor in the Indian Ocean; in the Mediterranean, nor in the Red Sea. But within the tropical Atlantic belt the records of all islands and

cities, including Havana, is the record of every American city such as New Orleans and Pensacola.

Since, therefore, yellow fever has unquestionably its centre of development on ships in the tropical Atlantic, and is transmissible from the ship to the port and from the port back to the ship again, many intelligent and trustworthy observers, without prejudice and sophistry, have recorded the spontaneous development of the disease on the broad ocean. If the vast array of facts, carefully studied, indicate this to be possible and frequent, the acknowledged impossibility of tracing its land origin anywhere, indicates that I am fully justified in considering the disease in its inception as a ship fever.

Dr. Chaillé did not hesitate in his concise report to devote ample space to the denunciation of this view and of those who entertained it. He said "there were formerly many, and there are still some few, who, without personal experience or extensive knowledge of the special subject, look upon it in a much simpler light. They pronounce yellow fever a "nautical disease." Until the publication last October of Prof. Gamgee's work, entitled "Yellow Fever, a Nautical Disease," I was practically the only person who had since 1858 adopted and propagated the idea that yellow fever was a disease of ships at sea; and since 1839 I have been an active observer of six great epidemics in Mobile, New Orleans, Pensacola and Milton, besides an annual experience with many sporadic cases amongst the shipping in this bay and the United States marine hospital.

But Dr. Chaillé thinks naval surgeons best entitled to credence, and quotes Dutroulau, who was compelled to publish in 1851 that in *not a single instance* of many infected vessels did the disease "originate on board." This is a sample of the authorities and facts given "in order that the world might correctly judge the validity of the conclusions," and the Havana commission submits this "with great confidence to the test of logic."

There are not many physicians in this country who have access to French medical libraries, and Dr. Chaillé has not sought the references, numerous enough in the English language. His champion is Dutroulau, whose recorded *facts* and *main conclusions* Dr. Chaillé passes over in profound silence, with the object, I suppose, to use his own words, that "all just and generous men will view with liberal charity any such conclusions when preceded by and accompanied with "his own selection of data." I have not read Dutroulau's works, but I have before me the very able lecture delivered by Prof. Gamgee on the 16th of December, in New Orleans, on the possible extinction of yellow fever, and from which it appears that Auguste Frederic Dutroulau said in his thesis, published in 1842, that an indispensable con-



dition of the focus of development (*foyer d'emanation*) is its maritime attribute." Nay, more, the examples are not rare of vessels in which yellow fever has declared itself spontaneously in the open sea (*en pleine mer*), and without having communicated with any infected land." He then reports a typical case, too long to be quoted, which occurred in 1833 in the brig *Cuirassier*, aboard of which Dutroulau was the surgeon in charge.

In 1853, in the *Archives Generales de Medicine*, Dutroulau distinguished the endemic or land miasms from the yellow fever poison which shuns the marshes. He said, yellow fever is limited to a very short distance from the sea-shore, whereas paludal fevers are observed wherever marsh miasm occurs, and authorizes the conclusion that the miasma of yellow fever derives one of its essential characters from the influence of the sea. More recently still, and in the very work quoted by Dr. Chaillé in his report, Dutroulau declares that the essential and primary cause of yellow fever is localized on the sea—"an infection proper to certain maritime localities."

I am now actively engaged in the preparation of my work to be entitled "The Tropical Atlantic Plague, Yellow Fever," wherein I shall show how simple and grand the truth is, as revealed by history, coupled with a wide experience in several cities of numerous epidemics. The science of pathology is not so hopelessly without basis, as Dr. Chaillé indicated when he confounds plagues proper, the pure contagia ever propagated from pre-existing cases, such as small-pox, with diseases like yellow fever of localized origin. Anxious as I am to see the work of the National Board of Health flourish for the salvation of our country, I trust to show yellow fever is an endemic of no land, in the sense in which the word endemic is defined by the profession at large. Once this is acknowledged, and the mercantile marine purified, yellow fever must become extinct.

I am, sir, your obedient servant,

ROBT. B. S. HARGIS, M. D.

This was succeeded by the two following letters:

NEW ORLEANS, *March 31, 1880.*

EDITOR OF THE PENSACOLA ADVANCE:

DEAR SIR—Permit me to furnish Dr. Hargis a brief reply, which will be final, because if I had time and inclination, which I have not, to engage in a newspaper controversy on the problem of yellow fever, I despair of either receiving or conferring benefit by controverting an opponent who so much misunderstands, and, apparently for this reason, misrepresents me. This is not the less discouraging because



of my friendly and firm conviction that the misrepresentations are without intention or bad purpose.

My first letter was designed to show how groundless was Dr. Hargis' charge against the National Board of Health, to indicate how little provocation he had for denouncing with strongest condemnation and consigning to the vengeance of "the next decade" his brother physicians,—animated by scientific zeal, integrity and knowledge, possibly as great as his own, and to intimate that he had misrepresented his true cause for his "strongest condemnation." His last letter, of March 27, fully confirms my positions in reference to these three points, and I very much regret that additional misrepresentations, some implied others clearly expressed, should tempt me again to weary your readers.

For my own credit, and the honor of the position held, it is to be hoped that there has been misrepresentation (implied) as to my "diplomacy" and "lack of precision in the use of scientific language," as to my "specious arguments based on false premises," as to my ignorance of the yellow fever literature of the English language, as to my being "blind or deaf to the clearest evidence," and as to my woeful deficiencies as an "enlightened pathologist." I have long taken an humble part in medical literature, but am unaccustomed, and intend to remain unaccustomed, if silence will assure it, to such implications.

Dr. Hargis persists in misrepresenting my views as to that ill-defined word "endemic," and upbraids me for the use of it with "mystery," but if used in a manner mysterious to him alone, am I responsible? He misrepresents the value I attach to Dutroulau's evidence, and he misrepresents—apparently misled by another—that evidence itself. He misrepresents my views as to the relationship between yellow fever and such diseases as small-pox, failing here, as elsewhere, to understand even in what particulars I agree with him. Finally, he misrepresents my own and the correct signification of the word "origin."

Even should he accomplish the Herculean feat of convincing others than himself "how simple and grand the truth is revealed by history, coupled with a wide experience in several cities of numerous epidemics (whatever this may mean) and that yellow fever originates on ships, he would then have done no more than prove *where* the disease originates, not *how* it originates, nor *what* the poison is, and would be just as far from the true origin of the disease as he would be from the true origin of a cat,—after having conclusively demonstrated that some cats have made their first appearance in his stable. Would any one discussing the "origin of man" deem himself answered by even the most conclusive proofs as to the *mere place where* man made his first

appearance? In such sense did I very manifestly use the word origin, and not in the sense misrepresented by Dr. Hargis.

If having thus specified the more important misrepresentations, Dr. Hargis should fail to appreciate them, then one of two things : Either this failure to appreciate will be his misfortune and not my fault, or my ignorance of the English language is so dense that I ought to refrain from abusing it further, to the vexation of Dr. H. and of your readers. Therefore I decline to enter into long and tedious explanations for the purpose of exposing in detail the misrepresentations which have been specified. Should any of your readers desire such explanations, they can find those most essential by a perusal, more critical and thoughtful than Dr. Hargis has given, of that very report which, in his opinion, "deserves the strongest condemnation." On the other hand, since the majority of your readers do not probably care a button about either the subject or misrepresentations, or explanations, they will rejoice that I hasten to subscribe myself,

Yours, very truly,

STANFORD E. CHAILLÉ, M. D.

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TO THE EDITOR OF THE PENSACOLA ADVANCE :

SIR—Dr. Chaillé has anticipated a resolve which I had formed, that any technical discussion between us should cease in the public press. The medical profession shall judge who it is that "misunderstands" or "misrepresents." Argument and evidence can only be fairly met by argument and counter-evidence. I seek enlightenment, whilst standing firm by the knowledge so far gained.

Dr. Chaillé attempts to fasten on me some charge against the National Board of Health. So much depends on comprehensive and uniform sanitary measures, that the influence of the medical profession should stand pre-eminent in the nation's councils, and no one shall excel me in fostering a just confidence in its chosen representatives.

Fortunately, Dr. Chaillé assumes the personal responsibility of defending views which I condemned, and data which I rejected as without foundation. He defines our relative position. I do not condemn the entire Havana Commission. Far from it ;—the researches of Dr. Sternberg promise to result in great good.

When refusing to hamper the operations of the National Board, by opposition before Congress, it was desirable to indicate that my action was not dictated by personal preference, but simply by a sense of public duty. In my letter to Dr. Le Hardy I held myself free to take exception to that which I deemed pernicious, whilst supporting the ex-

cellent general work of the Board. My Pensacola friends must have thought, for many years past, that I was fighting the winds, so constant and earnest have been my utterings on the subject of yellow fever, and its possible exclusion, not only from America, but all other lands. The time has come to show whether the opportunities enjoyed have been wasted, or whether another century must witness periodical outbursts of malignant pestilence, owing to some agency which mysteriously haunts the minds of Dr. Chaillé and others, as beyond the ken and grasp of man.

Dr. Chaillé requires, as the only measure of safety, non-intercourse as absolute as possible. He declares the ships are primarily contaminated from the shore, catch the *disease from land*, whereas all yellow fever history points unmistakably the other way.

I have a higher regard for your readers than to think, with Dr. Chaillé, that the majority "do not care a button" about this subject. Attempt to fence in the Gulf or other ports, by impenetrable barriers, and then we may see what the non-intercourse programme implies. Our merchants care not less for Pensacola than the New Orleans citizens for their port. In the last they are raising one hundred thousand dollars this year for sanitary purposes to avoid the inconvenience of useless quarantine and non-intercourse. They are led in this by an experienced physician, Dr. C. B. White, whose influence in such a community does honor to the profession, of which he has long been an energetic member.

I shall in due course announce in which of the medical journals my views as to Dr. Chaillé's writings are to be amplified, and meanwhile I subscribe myself,

Your obedient servant,

ROB'T B. S. HARGIS, M. D.

The charges and declarations made by Dr. Chaillé, in the foregoing letters, are of far too serious importance not to be fairly and fully met by arguments and statements, which I respectfully submit to the judgment of the profession. In the first place I shall speak of the

#### ENDEMICITY OF YELLOW FEVER IN CUBA.

Dr. Chaillé says it would be difficult "to cite a single Cuban authority of any weight in medicine," in opposition to the view of the endemicity of yellow fever. To speak more plainly, Cuban physicians advocate "local origin,"



and being in this, in my estimation, a quarter of a century behind the time, it is essential that the National Board of Health be informed of the actual and substantial grounds for entertaining such an opinion.

Dr. Chaillé insists on the great value of experience on this question. No one doubts it; but the most skillful hygienists and epidemiologists are not the busiest practitioners who visit the sick. I have been familiar with yellow fever for forty years, but have learnt that sanitarians who have never witnessed an epidemic can grasp the practical questions better than I can. An episode in the history of American medicine may prove instructive and a warning to writers who belittle their adversaries. When Philadelphia was scourged by yellow fever in 1793, Dr. Chas. Caldwell was a truly brilliant student of medicine. He was the first and last in charge of the city hospital and has described, in a graphic manner, the dread of contagion which caused medical men and students to fly, thus rendering assistance scarce. The nurses were few and inexperienced. The provisions and arrangements of the hurriedly prepared asylum were in all respects limited, crude and insufficient. In fact the whole establishment being the product of but two or three days labor "was a likeness in miniature of the city and the time, a scene of deep confusion and distress, not to say of utter desolation." "No apartments being yet prepared for the use and accommodation of the medical assistants; I was obliged," says Dr. Caldwell, "to eat, drink and sleep (when indeed I was permitted to sleep) in the same room in which I ministered to the wants of the sick. And not only did I sleep in the same room with my patients but also at times on the same bed," receiving occasionally "on some part of my apparel a portion of the matter of black vomit, and I was inhaling the breath of the sick, and immersed in the matter which exhaled from their systems, every hour of the day and night." He often woke to find

his patient dead beside him. Under these circumstances Dr. Caldwell abandoned his belief in the contagiousness of yellow fever and published the fact. At a meeting of the Philadelphia Medical Society, in the fall of the year, he redeemed a promise made to Dr. Rush and read a paper on the domestic origin of the disease. He was opposed by several who had left during the epidemic, and in the most dexterous manner he said, "Yellow fever has but just disappeared, the miasm productive of it having been destroyed, as it will always be, by the occurrence of cold weather. My wish, therefore, sir" (addressing the Chairman), "is to hear from the gentlemen arguments in favor of its foreign origin, drawn from what they have learned by their recent intercourse with it, in the way of observation as men of science, and of experience as physicians engaged in the treatment of it."

"This I knew," says Caldwell in his autobiography, "would be gall and wormwood; because not a single individual who, as yet, had contended, in the present debate, that yellow fever was an imported complaint, had ever seen a case of it. They had all hurried into the country on its first appearance in Philadelphia, and had now but just returned, to instruct the community, including those who had met it, contended with it, and studied it, as the mystery of its origin."

Dr. Caldwell has, and many since his time have, overlooked the fact that, as with a statesman defending a nation, who controls generals, or distant fields of battles, though himself not a warrior, so in epidemic times the calm and broad views of the events at many points, in a country so vast as this, give, the skillful scientific pathologists, a clearer perception than the local observer of the adverse influence he has to antagonize in order to limit the spread of disease. It is thus also with the logical mind building up a sound conception of the nature of plagues. He cannot afford to

overlook anything, or any one in the past, and, with far greater justice can we to-day, than ever before, apportion to Chisholm and Bancroft, Hosack and Rush, Chervin and LaRoche, the praise or the blame for the truths or errors they promulgated. Who more than Caldwell and Rush could vaunt a *practical experience*, and being trusted by all in the afflicted city, they imposed on the stricken a course of blood-letting and the famous "ten and ten dose of calomel and jalap?" Perhaps there is no living American who has seen more yellow fever than I have since 1839, so that I am entitled to encourage our unprotected hygienists, especially in the North, not to risk their lives, to add weight to their opinions, whilst engaged in the laborious duty of culling, for the common good, from the experience of centuries. Nay, more, the events recorded by unsophisticated minds, by young physicians, when careful note takers, rather than burdened with years and theories, will often be found to bear the impress of the most solid fact. This is conspicuously the case with the greatest of the authorities quoted by Dr. Chaillé, viz., Dutroulau.

We can look back to-day with composure and competent perception to the many fruitless efforts made to indicate the localities and circumstances charged with the "domestic origin" of yellow fever on American soil. The question, settled early by Tully and others of the broad line of separation between endemics, or malarial fevers of marshes, and the pestilence carried North annually right into the ports on the Connecticut, is still feebly agitated and periodically demolished. Land malaria and filth on land are charged with the local production of yellow fever only by those who do not read and cannot observe.

The "exotic origin" of the malady may be said to pervade the American medical mind as the fashionable opinion of the day. So far so good; but some land must be charged with the odium of developing this pestilence, since its local-

ized character is too striking to be ignored, and hence the importance of Dr. Chaillé's remarks concerning Cuba.

A very common, and amply justified, opinion is that yellow fever poison develops and propagates outside the human body, and it has been declared by such authorities as Jewell that "there is no adequate reason whatever for believing that it attaches especially to, or is multiplied in, the bodies of the sick." The latest views promulgated tend to oppose the idea of the spontaneous origin of yellow fever anywhere. In a clinical study by S. M. B., in the April number of the *American Journal of the Medical Sciences*, it is stated that yellow fever "is reproduced chiefly, if not wholly, within the body." The indispensable "germ" spreading from man to man is at the root of this opinion, but I agree with Mr. Gamgee that "there is no foundation in *fact* for the plausible explanation of the propagation of the disease by the reproduction of the lowest organisms."

With all these conflicting declarations before them, the National Board of Health very naturally sought to determine whether Cuba constituted a primary and permanent home of yellow fever. In other words whether it is endemic there as it was once supposed to be in New York. In attempting to settle this question Dr. Chaillé, who has acknowledged his responsibility for this portion of the Havana Commission Report, neglects the common signification given by medical writers to the words *endemic* and *endemicity*. He says that "the word endemic will not be used to imply that either this, or any other disease, which there may be occasion to thus designate, owes its origin *necessarily* to local causes." Then why use the word at all, other than to indicate a disease peculiar to the people or the nation? That endemicity in Cuba in relation to yellow fever is in a temporary or controllable condition is indicated by the statement that "the day may come when the people of the United States will demand whether their



welfare would not be best promoted by suspension of intercourse with Cuba during certain months of the year." Since small-pox would probably be found "as exceptionally and almost as severely prevalent as yellow fever, it has also become endemic in Cuba," and to fix beyond peradventure Dr. Chaillé's position, the following statement may be noted: "It should not be forgotten that while none of the causes referred to suffice to always explain all the variations in the prevalence of yellow fever, during certain seasons and years, yet that this *terra incognita* of this disease is also a *terra incognita* for small-pox, malarial, and other diseases. Science has no right to expect explanations of the mysteries of the one, while still unable to solve the similar mysteries of other much more familiar and widely prevalent diseases."

Small-pox has its home wherever man can live and be approached by other men. Malaria has a strictly defined boundary, as its source, as much as a river has in a mountain. Drain the land within these limits and malaria disappears. Relapsing fever originated during the great Irish famine, and travelled across the Atlantic. The geographical distribution of yellow fever and its relations to commerce, wars and revolutions, are illustrated by an array of facts not to be surpassed in epidemiology and only equalled by the abundant data possessed regarding Asiatic cholera.

Dr. Chaillé has but sought to justify the utter confusion in which, to say the least, an irregular use of the words endemic and endemicity, has created in the minds of readers of his preliminary report. Since he objects to Webster's definition after Hoblyn, I have searched a few of the principal authorities whose works I possess. Dr. C. B. Williams in his *Principles of Medicine* settles the question as understood by the whole profession. He avoids the expression by certain causes "inasmuch as this involves a hypothetical signification of their modes of action." Endem-

ics he says "are instances of disease which may be said to dwell among *the residents* in particular spots; hence they are called endemic *in the people*." His examples are *ague* and *goitre*. Other diseases are not confined to particular localities "although they infest some more than others."

\* \* \* \* "They attack a whole district, a whole country—nay, almost a whole hemisphere. These are called epidemics, like a blight or pernicious influence blowing on the people."

Dr. Aitkin says, in his *Science and Practice of Medicine*, "endemic influences result from those conditions or agencies peculiar to a locality which favor the development of various miasmatic diseases, and may thus account for their sudden origin. Such diseases are then said to be *endemic*."

Dr. Roger S. Tracy, in Dr. Buck's *Hygiene*, speaking of endemic diseases, says: "The type and exemplar of such diseases is the malarial fever, which takes upon itself so many forms and causes so much misery and so many broken constitutions in the West and South-west, and which shows itself with something less of intensity at various points along the Atlantic coast. Typhoid fever can almost be called endemic in the beautiful valley of the Connecticut, and tetanus in a certain district of Long Island. These diseases are due to local causes, and it is altogether probable that these causes will at some time be ascertained; and when they are once known, means can be adopted for their extinction."

Dr. Dunglison, in his *Dictionary on Medical Science*, says "an *endemic* is owing to some peculiarity in a situation or locality. Thus *ague* is endemic in marshy countries; *goitre* at the base of lofty mountains, etc. Some authors use the term in the same sense as epidemic."

Dr. Daniel Drake, than whom it is not easy to quote a more precise author, whatever opinion we may form of his conclusions, anticipated, many years since, in his ætiological

deductions, all that Dr. Chaillé implies when speaking of the endemicity of yellow fever in Cuba. Dr. Drake said, "whether the yellow fever of Havana originated there, or was introduced from abroad, we may speak and treat of it as a disease of that city; seeing that, although it is not extensively prevalent every summer, it is never absent, and thus, if not a native, *is a naturalized endemic.*"

Now what influence has Dr. Drake exercised on the Profession in relation to yellow fever? The hopeless confusion he launched the subject in, after the sound teachings of Tully and others, may be gleaned by the following. He said: "Is yellow fever, then, it may be asked, merely the remittent autumnal fever with which we are all familiar? The answer, according to these views of its origin is, that it is one of the varieties, as a tertian fever is another." That is where alone *endemicity* leads us, and the National Board of Health undoubtedly wished to know if the West Indian Islands were the plague spots whence yellow fever sprang, and was annually propagated. In other words, whether the *source* of the great stream of yellow fever, like the source of a river in a mountain, was not absolutely restricted to islands, of which Cuba is a type.

Dr. Chaillé has favored us with many useful facts and inferences relating to yellow fever. Every one of these may be cited as evidence against *the domestic origin* of yellow fever in Cuba, in plain medical language, against its *endemicity* in that Island. He says that "knowledge of nature's laws, of disease, of yellow fever particularly, and of other subjects relating to the present topic, convert the suspicion, justified by scanty historical records, into an absolute conviction that yellow fever was at least worse, in some of the West Indies with their first settlement by Europeans." No record or tradition lends the slightest support to the view that yellow fever reached the West Indies Islands before the ships of Columbus and his followers. Indeed, the



first epidemic of yellow fever, that probably ever occurred in this world, was that carried to Barcelona in 1497 by sailors returning to that port from the second expedition to the "Indies." The second epidemic of which we have any approximately reliable notice, was that recognized as a "new disease" which was named "*coup de barre*," from the intense rachialgia, and which was introduced into the Antilles by the ship *Le Bœuf*, from Rochelle, in France. Neither the West Indies in the Barcelona epidemic, nor Rochelle in the first invasions of the New World, had anything to do with the probable development of these epidemics, which, like so many others, in my opinion, emanated from the tropical Atlantic.

But we may pass from inferences and suppositions which, however interesting, have only a very limited scientific value, and truly does Dr. Chaillé remark that, "though Spanish literature is unusually rich in valuable histories of Cuba, no allusion has been found justifying even a suspicion that yellow fever was known in the Island from 1655 to 1761. On the contrary, repeated references are made to the remarkable salubrity of Havana, and the non-existence of any exceptional or devastating diseases during this interval."

That Cuba is naturally a most healthy Island is proved by Dr. Chaillé's statistics, which indicate, in the special case of Havana, a salubrity equal to the finest European and American cities, if "endemic yellow fever," "endemic small-pox," and phthisis in people seeking a warm climate, are excluded. Not all the filth, so forcibly described, nor a tropical climate of the most definite character engender any plague there. As to yellow fever, it "habitually prevails in every place in Cuba from which reports were received, *provided these places are of any size or commercial importance*, and contain any considerable number of unacclimated persons to furnish food for the wide-spread poison."

No epidemiologist can misconstrue the meaning of the sentence I have marked in italics. An endemic attacks the few or the many which are subjected to the evil influences engendering such a disease. The very spot mentioned by Lind, in his *Diseases of Hot Climates*, as having proved fatal in the summer and autumn of 1766 to French settlers on the Escambia River is well-known to me, and frequently since, attempts at peopling that place have ended in the sickness and death of all who dared to sleep one night in that locality, in August or September.

#### ON ACCLIMATION.

I must also object most strenuously, especially when speaking of yellow-fever, to the use of the words *acclimation*, *acclimated* and *unacclimated*. All persons with slight differences of race predispositions are liable to yellow fever, however long they may have resided anywhere on land. Protection is only absolutely gained by an attack of the disease, which in some cases may be slight and barely perceptible. My objection to the word *acclimation* extends also to the true endemic, for a wide experience of these maladies has proved to me that people inhabiting malarious regions suffer more the longer they remain there, and the fatal form of yellow disease, a malignant paludal fever, as in the cane brakes of Alabama, ultimately develops.

Dr. Chaillé "proves how erroneous is the idea that yellow fever is especially and exceptionally severe in the shipping and harbor of Havana. He has not stated what my own extensive and very precise inquiries have indicated, viz.: that the crews of ships frequenting all West Indian and Gulf Ports are protected to the extent of full 50 per cent. Besides this, many a healthy ship escapes the disease just like a large number of even "unacclimated" people pass into Havana, and even Vera Cruz, every year without contracting the disease.

The subjoined remarks, indicating that yellow fever depends on something more than local filth, are instructive, and indicate that the yellow fever poison is an element superadded to defective hygienic conditions :

“There has been no intention to convey the idea that houses may not be found in New York and other American cities as foul as they can be, and, therefore, as foul as they are found in Cuba ; but in the former these evil conditions are seen as exceptions, confined to narrow, disreputable limits, while in Havana these conditions in the ‘homes of the poor’ are widespread and general. Moist, foul, stagnant air, confined low to the ground, is found everywhere, so everywhere can be seen the refuse of fruits and vegetable substances, furnishing abundant materials for decomposition, while numerous turkey-buzzards, roosting on the trees and house-tops of populous cities, sufficiently testify to ample supplies for animal putrefaction. These gross insanitary evils are as abundant in Havana, where yellow-fever always prevails, as in Canton and Bombay, where this disease never occurs.”

“There is one more subject which deserves brief notice in connection with air-polluting causes and with theories maintained as to yellow fever. Sanitarians were greatly offended by the burial of the dead of Havana in its churches until 1806, when the “cemetery of Espada” was established outside the walls. In the course of years the growing city surrounded this cemetery, and to this was again attributed, among other insanitary evils, a bad influence on yellow-fever. Overcrowded with more than three hundred thousand dead bodies it was closed in 1871, since when all the dead of Havana have been interred in the new “cemetery of Colon,” which is admirably located and too distant from the population to exercise upon it any evil influence. It is noteworthy that there has apparently been no abatement in the prevalence of yellow fever.”

## THE SHIP ORIGIN OF YELLOW FEVER.

Failing to determine the causes of endemicity and noting the perplexity as to what measure may be advocated for the benefit of Cuban Ports and the world trading with them, Dr. Chaillé tells us "that the people must be provided with means to become intelligent, enlightened (especially in hygiene,) prosperous, and sufficiently numerous to eventually gain both the knowledge and the power necessary to correct their insanitary evils. This is not only the best, but the only means. Until their accomplishment (which the present generation will not live to witness) Havana will continue to be a source of constant danger to every vessel within its harbor, and to every southern port which these vessels may sail to during the warm season."

"While these are the conclusions of this Commission, there were formerly many, and there are still some few, who, without personal experience or extensive knowledge of this special subject, look upon it in a much simpler light. They pronounce yellow fever "a nautical disease," and seem to believe that if means were adopted to rid ships and harbors of the poison generated, as is alleged, and contained in them, there would be an end to it. Since this view leads to practical results of great sanitary importance the facts in the matter have been carefully examined in Cuba, and also the records of the facts in other of the West Indies."

"*The only means*" seem to me as indefinite and unattainable as any ever proposed, and Dr. Chaillé's declaration concerning the former, "many" and "still some few" who "without experience or extensive knowledge of this special subject," pronounce yellow fever a nautical disease, is in direct and flagrant defiance of all history.

For twenty-five years I have sought allies and supporters for this truth from amongst writers of the past and my con-



temporaries. I left the Medical School of Louisiana a believer in "local origin" on land like every disciple of Stone. The Northampton, direct from England, infected New Orleans in 1853, and I purposely visited that ship to satisfy myself as to its condition. A new light dawned on me, but it was five years before I felt warranted in declaring, in direct opposition to every authority I could consult, that yellow fever was never a land malady, but always localized in its source on sea in the tropics. The few who have hinted at the nautical origin of yellow fever, have been men of exceptionally large experience. The individual cases of ships in which disease has been unmistakably developed are, as a rule, singularly well recorded. They offer a striking contrast to the many, but utterly baseless assertions concerning the sites and circumstances of local origin on land. They have been recorded by contagionists and non-contagionists alike—though we must admit that many of the earliest and most instructive examples, relating to local origin in ships, were published by physicians who believed the malady would originate anywhere, under definite conditions of heat, moisture and foul effluvia.

Those who have written with fairness and learning on yellow fever, have not ventured to explain away the precise facts which abound, but which somehow failed to create the impression needed to formulate the law that "yellow fever is a naval malady of the tropical Atlantic, communicable to, and capable of, persistence on shore, but never presenting the feature of a land plague like cholera." It was most disappointing to me, at first, to notice that the ablest authors, in discussing the source of yellow fever, failed to appreciate this one fundamental idea, an adequate statement and demonstration of which was never in print until Mr. Gamgee treated the question last summer with learning and thoroughness. When he first broached the matter to friends, and before he knew of my writings, he was met by expres-

sions of astonishment, precisely as I have been, even as late as at the Nashville meeting of the American Public Health Association. It is, therefore, a little surprising to find an attempt made to discredit the originality of this conception, and to overlook its important bearings on the present medical era.

It is true that towards the close of the last century Dr. Griffiths of Philadelphia, in a letter to Dr. David Hosack, used the following words: "The disease called yellow fever—but which I call the ship fever of the tropical climates." It is not clear that he did not ascribe some influence in its production to the West Indies where, according to La Roche, Dr. Griffiths believed the fever was not apparently contagious, "but he thought it became so during the passage from the Antilles to the United States." Hosack, of all men, came nearest the truth, for he considered yellow fever to be the disease of northern men removed to the tropics, but he had not the data before him to limit its precise centre of development. He classed it with plague, typhus, etc., apart from the pure contagia, "which are communicated exclusively by contact and under all circumstances; the diseases of this separate class, which includes yellow fever, he points out as '*specific*,' but only in general communicable, through the medium of an impure atmosphere."

Though Dr. Rush ascribed the fever of 1797, at Philadelphia, to a foul ship, direct from Marseilles, and an outbreak at Kensington to a ship from Hamburg (both very possible occurrences in the days of sailing ships striking a southern course), he did not suspect that the disease was a pure ship fever. Dr. Caldwell's report of the sloop Mary, which had visited no sickly port, and produced yellow fever amongst several persons exposed to the effluvia of the hold after opening ports and hatches, was one of the earliest, followed at various intervals by carefully noted cases, a perusal of which, as La Roche asserted, "will fully sustain the

opinion of those who ascribe, in very many instances, the appearance and spread of the disease to morbid effluvia generated in the vessels themselves." Dr. Ferguson, of Demerara, Audouard, Dickson and Alison have spoken of the occasional origination of the fever on board of ships. Copland saw it on a slave ship and thought it might be produced where masses of human beings were congregated in the hold of a vessel. He said: "If this opinion as to the probable origin of the infectious poison be not admitted there is certainly no other deserving greater confidence."

Dr. J. C. Faget of New Orleans, in his *Memoirs on Yellow Fever*, published in 1859, accepts as most reasonable, Audouard's views concerning the outbreak of yellow fever in ships at sea (*même en pleine mer*) as Dutroulau had declared in 1842. In the New Orleans *Medical News and Hospital Gazette* for January, 1859, will be found a letter in which I say, *for the first time*, that "yellow fever is neither indigenous to this country nor a native of any foreign clime," and "that it is in the holds of vessels within the tropics a certain period of time, that are to be found the nidus in which the germs of yellow fever are engendered, developed and propagated, and from them, under favorable circumstances, disseminated on reaching inhabited shores, within certain degrees of latitude."

Last year Professor Gamgee entered into greater details and pointed to the conditions which localized the disease, in its origin *de novo*, in the calm belts of the Atlantic, mainly on the western half, the originally infected ships infecting land and other ships, until the full sway of malignant epidemics might be witnessed under similar conditions in the West Indies and in our sea ports. In his first New Orleans lecture he defined the malady as a pelagic or ocean malady confined within definite limits in the Equatorial Atlantic, and whilst communicable to points most accessible to maritime commerce, never penetrating as a plague in or

across continents. He likewise indicated how utterly without basis was the "germ" theory of the disease. The supposed vital cause, demanding progressive generation throughout time and restricted to reproduction like animal and vegetable forms is not only hypothetical, but at the root of a new hypothesis which represents the latest fancies of yellow fever pathologists, who refuse to acknowledge the possibility of the spontaneous development of any disease.

They endow with life the essential elements of all transmissible maladies. In this I believe them to be in error.

Dr. Chaillé must kindly enlighten us as to the *many* or *few* who, inexperienced or "without extensive knowledge of this special subject," have pronounced yellow fever "a nautical disease." I challenge him to support this statement by anything like adequate proof.

#### DR. CHAILLÉ'S EVIDENCE.

Three columns of the concise Report are devoted by Dr. Chaillé to the origin of yellow fever in ships and harbors, in order to state the "facts" at variance with the view. The vast importance of setting this point at rest is indicated in a very peculiar way by Dr. Chaillé. He says:

"There is a final deduction of much scientific importance derivable from the conclusion that yellow fever never originates on ships. This deduction will be better understood if preceded by the statement that the facts occurring within an infected place, and bearing upon the questioned transmission of yellow fever, can be as well explained by infection of locality as by infection through the movable things and persons in such locality; and that therefore these facts as they occur outside of infected localities must be more particularly relied on to solve the question. Now, vessels—because of their restricted limits and of their small and more readily observed contents and population—are the



places which present the most numerous and favorable opportunities for the solution of all questions which relate to the modes by which the poison is conveyed to such places, as well as from thence to other places, and also to the conditions necessary for the propagation of the poison."

It is clear from this and all other statements that Dr. Chaillé considers "the facts now presented" \* \* \* "tend to prove beyond question that the poison of yellow fever is on the shore, and *not in the waters of the harbor.*"

Now what are these convincing facts? The reader is requested to peruse the three columns of evidence and to assure himself that my summary or criticism as follows, is justified. Dr. Chaillé begins like many recent writers on yellow fever, vaguely discrediting inexperienced writers and observers, without enabling his readers to determine whom he is hitting at, or for what reason an invisible enemy is demolished.

"Naval surgeons, and especially those among them charged with marine sanitation, are, of all other medical men, best entitled to credence and confidence in this matter. Evidence from two such French witnesses will first be submitted, one of these being A. F. Dutroulau, 'premier médecin en chef de la marine,' who had more than twenty years' personal experience of yellow fever, particularly in Martinique, and the other being Beranger Feraud, who held the same high post in the French navy, and also had extensive personal experience. Each of them was charged at different epochs with the duty of preventing yellow fever on ships and in seaports; and while surrounded for years by special opportunities for observation, was required by imperative official duty to study particularly the causation and prevention of yellow fever."

The evidence of Dutroulau, as given in my first reply to Dr. Chaillé, is conclusive that this authority considered the essential element in the development of the yellow

fever poison as of maritime origin. And if Dutroulau's facts, as well as some later theory, had been quoted by Dr. Chaillé, the readers of the concise report would have had some means of independent judgment. To choose what suits a simple hypothesis from the pages of a voluminous author, is not quoting "facts" as the fair premises of a syllogism.

This is not all. Beranger Feraud's "facts" are not cited for he has none; he does not deem it necessary to specially consider the subject. Dr. Chaillé quotes him as insisting on the isolation of ships from land, and states a fact that "the common experience of mariners in the ports of the West Indies teaches them especially to avoid the shore." It is truly astonishing to find such a statement, which I am almost tempted to style puerile, published in disproof of the ocean origin of yellow fever. Is it not certain that for one ship, in which the disease springs *de novo*, scores are infected, in epidemic times, in the same way as houses are in seaports?

Dr. Chaillé does not say that he personally visited the shipping and acquired a knowledge of the striking differences between foul ships carrying disease to Havana from the ocean, and the recently infected ships smitten in the harbor. Had he done this, he might have had some *facts* the nature of which I shall specially detail in my work to be shortly published.

The next authority, quoted by Dr. Chaillé, counts for another authority but, so far as the report is concerned, not for another fact against the position I hold. Dr. Chaillé says :

"Dr. Fuzier, a French army surgeon in high official position during that most favorable period for observation, 1861 to 1865, when France occupied Mexico, and at that most favorable place for observation, Vera Cruz, denies absolutely the spontaneous origin of yellow fever on ships."

"Such is the evidence derived from the highest French authorities."

Intending to analyze all these facts, the abundance of quotations is to avoid the slightest chance of misinterpretation. "The experience in the Spanish navy is not less emphatic" than the French. Dr. de Caneda has "*frequently*" observed the first cases on board ships amongst "persons whose duty calls them oftenest to the shore." Could he have said "*always*" instead of "frequently," he might have been of some use to Dr. Chaillé. Dr. de Caneda and a special commission appointed to report on the questions propounded by Dr. Chaillé, could, in truth, only declare what every tyro in a busy seaport in the tropics knows, where navies congregate, that those most liable to contract yellow fever are those who inhabit the arsenal, and who frequent the wharves. Dr. Chaillé does not seem to have heard of the speed with which any prudent Commander in the American navy clears out of a port when an epidemic is threatened. That the shipping in harbor forms part, and a most dangerous part, of an infected seaport has been well and widely known for over three-quarters of a century. Did not Lind show that healthy ships put to sea might save those who boarded them, and that "during the sickness at Cadiz and Pensacola, the removal of the sick into ships which lay at anchor, in a pure air, produced the same happy effects?"

Has it not often happened that a ship has crossed the Atlantic with a healthy crew, notwithstanding a protracted sail in the tropics, and yellow fever breaking out when the ship was being unloaded, the "vile" climate of the port has been charged with engendering a poison which had been days and weeks in process of development within the vessels? As Humboldt (one of the despised philosophers, if we are to shun the teachings of those who have not studied yellow fever clinically), said, "as malignant fevers are



easily engendered, amid a large crew, crowded together in filthy vessels, the commencement of an epidemic dates pretty often from the arrival of a squadron." The people affirm that the disease has been imported from a neighboring port, as it often, but far from invariably, has been.

Reverting again to Dr. de Caneda's commission, Dr. Chaillé tells us they had "not sufficient time to collect statistical data in detail," for a "demonstration beyond question" of that which needed no demonstration, for it did not in the least bear on the naval origin of yellow fever in the tropical Atlantic.

"Fortunately, however," says Dr. Chaillé, "that able and zealous officer of the United States, Dr. D. M. Burgess, \* \* \* \* was enabled to collect statistical facts in detail, and to present a very valuable tabular statement of these facts." The table is reserved for the final report, but Dr. Burgess asserts "that those vessels which lie at wharves suffer incomparably the most. Of 31 vessels discharging at wharves, in July, August and September, 1879, only one in 15 escaped infection. "The liability to infection in this harbor is in an inverse ratio to the distance at which a vessel lies from wharves and habitations." Moreover, there "is a striking difference in the chances of infection, when making comparison between the wharves and the open bay." Dr. Burgess has "no hesitation in saying that the nearer the vessel is to wharves and to habitations, the more it is exposed to infection," and with a caution, which may be commended to Dr. Chaillé's special attention, he concludes by saying that "statistical records are very desirable on all these points."

Such is the *evidence* (?)—*the whole evidence—every fact and opinion*—enumerated, which precedes the extraordinary conclusion of Dr. Chaillé, which he states as follows: "While all freely admit that yellow fever finds a favorable medium on vessels, especially in the parts where air is



confined, for the propagation of its poison, the facts now presented are totally irreconcilable with the theory of the spontaneous origin of this poison on ships." It is quite impossible to give adequate expression to my astonishment at the complete misconception manifested by Dr. Chaillé, of the ordinary meaning of the words "*facts*" and "*evidence*." He has simply ignored anything and everything bearing, even incidentally, on the possible origin of yellow fever in ships. Going out of his way to quote French Naval Surgeons, could he not find ample material ready at hand in the Annals of our Navy Medical Department? I happen, by chance, to have on my table the Sanitary and Medical Reports for 1873-4, by officers of the U. S. Navy, in which Dr. Thomas N. Penrose reports on the yellow fever on board the U. S. S. Ticonderoga, at Key West, Fla., August, 1874. This is one of many cases which will furnish food for reflection.

That the Profession may distinctly understand the practical result which I anticipate from proving that yellow fever is the *ship* fever of the tropics, I may at once state that its malignancy, when first introduced under circumstances favorable to a high mortality, has a natural tendency to diminish. In a warm climate and in warm dwellings, it may remain over one, two or three years, but, as with cholera, its known tendency, even in the West Indian Islands, is to extinction. Fresh poison and fresh people are indispensable to its permanence. According to Dr. Chaillé, the endemicity in Cuba will insure the persistence of the disease, whatever measures might be adopted with the shipping. I do not agree with him, and believe that the exclusion of newly-introduced yellow fever poison could restore Havana to the condition in which it was prior to 1761, and that, at the worst, the local developments would admit of sanitary control. The history of Jamaica, Marti-

nique, Guadaloupe, and the Bermudas teaches this, if it teaches anything.

Fierce in his attacks on the exotic origin of yellow fever, La Roche, armed at all points by evidence which he reproduced fairly, whilst attempting to bend it in support of his views, admitted the absolutely incontestable character of the records of vessels "in which the yellow fever has truly originated and prevailed." I ask Dr. Chaillé to analyze the numerous cases quoted by La Roche and explain how they can possibly be erased from the pages of reliable history. In the light of augmenting experience, apparently incomprehensible and irreconcilable facts came to be understood, and we now know how it has often happened that an epidemic, in an American or European port, has succeeded the arrival of a vessel, with a clean bill of health, from a port which could not possibly be infected. Even taking such a case as that of the ship *Eliza* accused of carrying yellow fever into Philadelphia in 1799, La Roche was not justified in dismissing all that was said, "inasmuch as she came from Leghorn, where the disease did not exist at the time, and has as yet never occurred." Hundreds of cases prove that she might develop the disease on the voyage as the Norwegian ship *Skebladna* did in 1877 on a passage of sixty-three days, laden with sweepings from the streets of London, and long becalmed in the latitude and heat of Fermandina.

The question before us stands thus. The old doctrine of "domestic origin" of yellow fever, wherever this occurs on land, has been abandoned by Dr. Chaillé, who dared not attempt to prove it in Cuba. There the endemicity of yellow fever—whatever that may mean—is precisely analogous to the endemicity of small-pox. Dr. Chaillé, without supporting his opinion by facts, nor evidence of the slightest importance, actually neglects the numerous statements of his leading authority, Dutroulau, that the essential ele-

ments of yellow fever is of maritime origin. He denies that yellow fever ever originated *de novo* in ships. Where then is its source? No where? The whole history and the geographical limits of the disease point to this as simply absurd.

But for the length of this article I would have commented on Dr. Chaillé's extraordinary declaration, as to the impossibility of defining the sources of the special diseases he mentions. Resting satisfied with one example—the case of human tapeworms—what more do we require to learn, with a view to limiting their reproduction, than that they are always derived from eating raw meat. Measly pork is the immediate cause of *taenia solium*, and raw veal of *taenia mediocannellata*. Of all so-called fevers or epidemics, none present more clear and trenchant features than yellow fever, and its recognition, as an ocean pestilence, will contribute quite as much to the advancement of pathology as a science, as did the labors of the illustrious Louis in relation to the distinction between typhus and typhoid.

In conclusion I shall quote a few words from a philosophical lecture, just published in the London *Lancet*, on the History of Mental Medicine, by Ball. He tells us, in the first place, to respect our ancestors. In the second place, to observe well, and in the third, to be sceptical; and he explains the word scepticism as follows: "I do not mean by that, that morbid frame of mind which makes us receive all new conceptions with a vulgar irony, and which would become, in the long run, more destructive to the true interests of science than the most childish credulity. I mean by scepticism that negative virtue which consists in never accepting a fact without verifying it, an idea without discussing it, and which teaches us to yield only when the laden mind comes to bend beneath the burden of proofs"

\* \* \* \* "subdued only by truth."

## PRACTICAL HINTS

### RELATING TO YELLOW FEVER PREVENTION.\*

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The signs of the current season are such as I have known to precede yellow-fever epidemics. The history of the disease in this region is that there are years when the spread seems to be impossible, and there are others when all efforts fail to stem its course. The condition essential to an outbreak is the arrival of infected shipping; but from 1853 to 1861, I treated cases annually, in vessels in our bay, and coming ashore—made no disguise of my mission, without exciting surprise, much less fear. The virulent epidemic of 1853 originated in New Orleans on the arrival of the ship "Northampton," direct from Liverpool, with emigrants. False swearing failed to hide the deaths from yellow-fever, which occurred in the Carribean sea, or the cases presenting themselves in the gulf, and amongst newly landed passengers. I personally investigated this case and know that the vessel never touched land between Europe and New Orleans, and was protected from any other sources of contamination, than that incidental to traversing the dangerous Atlantic calm belts, under conditions favorable to yellow-fever development. The out-break occurred early in the season, in May, and we became the victims of an extensive epidemic, which had been introduced from abroad by an infected vessel.

It was this case of the "Northampton" and its resultant evils, which made me waver in opinion as to the possible

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\* From the Independent Practitioner, July, 1880.



land origin of the disease, and which modified the course of my subsequent investigations, observations and thoughts. The lesson was too striking to produce any but the strongest impression, and I soon familiarized myself with the contrasts exhibited by the numerous ships, exhibiting signs of having been poisoned in infected ports, and of those in which the disease had evidently broken out spontaneously. The many opportunities for study, in the eight years preceding the war, satisfied me, first, that the epidemic of 1853 had protected the majority of our people, hence the uncertainty of infection from communication with the shipping. Secondly, that the persistent gulf breeze, which renders Pensacola one of the most delightful cities in the South for summer residence, in all probability exercises a marked protective influence against yellow-fever, which revels in stagnant air. Thirdly, that the periods of high temperature are not necessarily the periods of greatest danger to human life; but cold nights succeeding hot days seem to check the discharge, from the system, of poison which might otherwise be forced out, and develop many cases of an aggravated type.

Fourthly, that in years when an excess of ozone in the atmosphere in spring is attended by almost epidemic and very severe catarrhs, we are apt to have an autumn with an atmosphere deficient in ozone, well adapted to favor the spread of yellow-fever. No example was more startling than in the condition of matters in 1878, when we escaped the epidemic by rigid quarantine. In no year have I noticed better defined indications, which personal experience causes me to reckon as of special importance.

We are having a repetition of 1878. I do not attempt to foresee the meteorological features of the succeeding months, but we have had the catarrhal fevers, and now we must look out against disease importation. No one is more conscious than I am of the slight scientific value of these gen-

#### 44 *Practical Hints Relating to Yellow Fever Prevention.*

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eralities, which are mentioned to direct inquiries and not to stifle investigation. As practitioners of medicine, we form personal opinions, from local knowledge which helps us materially in our work, and soon we shall have much greater precision and more rational deductions. I can, however, confirm an almost accepted truth in epidemiology, that the careful observer must watch the vital statistics of seasons preceding epidemics, to obtain useful etiological information. It is this which has inspired me with hope or fear, when witnessing the first instance of yellow-fever in the shipping in our port. In some cases I felt sure of exemption, and, in others, of fatal consequences.

Western Florida has furnished me with some singularly definite experiences, and whilst at one time witnessing the malady on the cleanest, driest and healthiest regions, as at Milton and Fort Barrancas, I can testify to the virulence of the malady in unwholesome and over-crowded dens. Here we have, fortunately in winter, a congregation of old hulks discarded for all other than the lumber trade; the many annually lost and reported waterlogged on the broad ocean, were not liable to yellow-fever. When the pumps have to be kept going all the time, it is difficult, but by no means impossible, to have fever developed. These old and defective vessels have afforded me personal experience, in summer, of the poisonous character of these emanations—for it was by concentrated ship poison I caught yellow-fever in 1853, after having past unscathed through a number of epidemics.

The malarial element on the Escambia, distinctly limited now as it was in the days of the French Huguenots, cited by Lind, cannot travel in ships at sea; agues cease and sailors improve every day after they leave port; but it is quite otherwise on vessels which have either caught or developed yellow-fever.

## OCEAN-INFECTED SHIPS.

The resident in a Gulf port having daily intercourse with mariners, learns that the suffering at sea in the tropics are beyond description. Hailing from any quarter of the globe, the oppressed crews refuse to go below, and lie about on deck, seeking some means of relief by the working of the ship, in the deathly stillness of the "calms." At times a man ventures below and the foul forecastle seals his fate. He is buried at sea, having died of a nameless disease, which I know from analogous cases, reported by intelligent captains and medical men, to be generally yellow-fever. But such a ship may enter our port, in ballast, without a death, and only an unusual sallowness and sickness of the crews. They go ashore and arrange for discharging their ships and for cutting ports to load lumber. The laborers and carpenters engaged in these operations are the first to succumb. They are specially susceptible and are often the first and only ones to reach the foul bilge. It is only those who have smelt, the combined sulphuretted hydrogen and fetid ammoniacal odors, who can appreciate the character of this poisonous gas, incapable of supporting combustion and animal life. Dead rats and roaches by the bushel are not rarities, and I have seen the active bubbling of gas on the surface of the bilge, supplying a constant quantity of putrid vapor, which effectually displaces or precludes the entrance of fresh air.

Ocean infected ships usually have rotton timbers, are not very leaky, come in sometimes from Rio with rock ballast, whereas, at others, they have a more dangerous ballast in the shape of coal, which is loaded in Wales to avoid the charge for relieving a vessel of other ballast.

The merchant ships of the future should have water ballast in perfectly tight tanks. Every part of the timber should be made impervious to wet and elements of organic decay.



A ship may have an infected hold, as the result of infection from land, so that whilst specifying the distinctive features of the condition of a sickly ship entering our ports, after having left any European port without possible yellow-fever infection, there are cases which may be confounded with it. Above all, we must state our positions fairly and truthfully, not attempting to prove too much, or reckless by disregarding sources of recorded facts, after the manner of recent writers, who declare the spontaneous origin of yellow-fever in ships impossible.

#### SIMPLE FEVER CARRIERS.

I have vividly before my eyes the "Fair Wind," a vessel now in this port, and which is not a yellow-fever ship, like those in which the disease has originated *de novo*. She came here in July, 1867, as clean, as pure and well equipped as any vessel I have ever seen of her kind. She brought the yellow-fever, and how? Amongst her crew, who contracted the disease at Kingston, Jamaica, before sailing for the port of Pensacola. After the discontinuance of the disease on board, and though disinfected in the uppermost deck, cabin, forecastle, &c., she opened her hatches and port holes and proceeded to load; not a single case occurred amongst the steamer's employés that could be traced to that source; the "Fair Wind" was a carrier, but not a generator of that malady.

Land infected vessels show signs of the disease usually after leaving an infected port, and rarely beyond the 4th or 5th day. When a ship has been ten days at sea or over, the chances are she has a foul bilge and will infect all the susceptible people who enter her.

Vessels from Rio coming to this port, if infected in Rio, show signs of the disease unmistakably south of the equator, and usually, south of the tropic of Capricorn. If the outbreak be delayed until the line is crossed, the permanent infection of the vessel, originating in the ship, is highly prob-



able. Nothing but familiarity with this subject and with shipping, can enable a sanitarian to appreciate the sharpness of these distinctions. The most puzzling complications are due to the vast difference between men as to constitutional pre-disposition. In yellow-fever, as in every disease, the study of diathesis, susceptibility, cause of immunity, may be placed in the first rank with a view to intelligent records.

Medical men especially connected with Southern ports, should, in tracing the history of a ship, obtain from the log an account of her course. A sailing vessel from Europe making for New Orleans, has to keep far out on the Atlantic and strike a southerly course, to avoid the opposing gulf stream, and this is a circumstance which has specially attracted Mr. Gamgee's attention, accounting for the rarer out-breaks on vessels leaving for Europe, than those coming from Europe or sailing into the gulf from the southern hemisphere. Every degree north that is past affords a relative protection, but the winds and currents which impel the vessels northward, exert a favorable influence on the health of the crew and passengers of ships at sea.

Indeed, it is under these circumstances that even susceptible crews suffer little, or not at all, in an infected ship, until she reaches a port and is opened out as in the celebrated case of the "Anne Marie," which left Nantes for Havana on the 12th of May, 1861, started healthy on her return voyage on the 13th of June, and seventeen days after leaving port had a case of yellow-fever on board. The disastrous consequences which ensued in and around St. Nazaire, in the north of France indicated that the northward travel of yellow-fever is not impossible, though ships are much relieved by stiff trade winds.

#### THE PERIOD OF INCUBATION OF YELLOW-FEVER.

Slowly, but surely, have my eyes been opened to the folly of confounding natural phenomena, in relation to disease.

I appeal to medical men to abandon the talk about "germs" until "germs" are proved to exist. I have sinned like my colleagues, but the evils attendant on the use of words which do not implicitly convey a truth, have taught me a lesson. It is a fact that the development of fecundated ova, the embryonic stage of every known organic form, demand specific and invariable periods of time. A female acarus lodging on the human skin, has to produce the germs to induce the general and appreciable signs of itch. This takes time. The poison of small pox requires a definite and almost invariable period of latency or incubation, to overwhelm a healthy person. Not so with yellow-fever. All the statements made in the past must be carefully scanned, and it will be found, that where a period of latency exists, between exposure to the poison and manifestation of its effects, this period is usually very brief and extremely uncertain. It varies with the cases and with the epidemics. The simultaneous manifestations of the disease in two or more persons exposed at the same time to the poison, often observed, may be explained on the principle of poison doses. There are many drugs given in like quantities to two similar individuals, at the same time, which produce a definite result at the exact same time. But strengthen the dose, and as with yellow-fever, the effect is immediate. From the earliest careful observations of this disease, produced by foul ships, on susceptible people in ports, the instant sickness of persons who have breathed the air of the hold has been noticed. I returned to shore from an infected ship in 1853, and remarked to friends who met me, that I was never so sick before in my life, as I was then. They gave me brandy; I rejected it and took another drink with like effect. All the premonitory signs of fever seized me at once, and next morning I was transferred to Milton, which I reached in an unconscious state, and remained so, more or less, for six days, with marked symptoms of yel-

low-fever. I convalesced on the seventh day from the time of exposure and commencement of sickness. The same season a carpenter, who left Milton as a refugee to escape yellow-fever, went to Coffee County, Alabama, and returned after the first frost. He went straight to the house of his late friend Trammel, who had died in the summer; the house had been kept warm and, after entering Trammel's bed, he was forthwith seized with the premonitory signs of yellow-fever and I treated him.

These are not rare though somewhat exceptional cases, and the experience in northern ports that people who have remained healthy five or six days at sea, may be regarded as free from the poison, is to a great extent indicative of the prompt action of the yellow-fever poison. The type of the disease varies within certain limits strikingly, but every time has the same uncertain invasion which is totally unlike the latency and invasion of true fever. The well-marked supra-orbital and other pains, with or without rigors, supervene in one, two, three or four days, and rarely longer.

These points have a most important bearing on the prevention of the disease, and, guided by them, we can readily understand how ship infection, the inevitable precursor to port infection, must be controlled, whilst the people carried are much more easily dealt with, owing to the brevity of the latent stage. In six years we have had no yellow-fever, but quarantine measures have been fairly carried out, and have sufficed in conjunction with natural tendency to exceptional atmospheric purity, thanks to persistent natural winds peculiar to this region of country.

Pensacola is by no means a model of sanitary arrangements. We might easily have efficient drainage, which we lack. We have plenty of space and porous soil, so that until the town is more populous and crowded, we may escape the visitations of typhoid and other fevers or great mortality from cholera, should it be introduced. We have

probably the healthiest city in the United States, and the infant mortality is exceedingly slight. I may define it a rural port, sheltered by healthy piney woods, and, thanks to the breeze, free from the heat diseases of inland, and of other cities on this and even more northern latitudes.

Well have I understood, as a resident in Pensacola, the remarks frequently printed on the salubrity of the West Indian Islands, such as Curacoa, and how exactly similar the history of yellow fever here is with that of any other tropical port.

#### PREVENTION OF YELLOW FEVER.

Every eye witness of a city, during an epidemic of yellow fever, has occasion to deplore the absolute helplessness of citizens and physicians in controlling the spread or anticipating the course of the plague. The well-founded opinions of the rarity of personal contagion, under certain circumstances, coupled with a large body of protected people, as in New Orleans, may be recognized as having materially favored the congregation of gossiping citizens in infected houses and localities, and thus, even by the well, the disease has been carried into many a house. The time was when, with less sanitary rule than is now enforced, a crop of yellow fever cases led the curious and the charitable, the friends and the strangers who had no reason to fear an attack, to congregate, especially at the beginning of an outbreak, just like people run to a fire.

The now acknowledged transmissibility of the disease leads to the imposition of wholesome restrictions. Nevertheless, taking a case like that of Memphis, depopulation and isolation failed to arrest the malady before frost. The reason for this is, in all probability, that the impure air, poisoned in almost unlimited masses, in hospital wards and houses, is free to move and passes laden from block to block and street to street, until the sparsely inhabited sub-



urbs and open country cut it short by abundant atmospheric dilution.

As a ship fever this malady is ever engendered by volatile matter, or matter suspended in moist, warm air, emerging when the hold is opened, to poison the forecastle, the cabins, adjoining ships and houses. A slight breeze hastens its transit, but often neutralizes it. It does not go far. A contaminated house on a wharf does not transmit by fomites, usually carried by man, the disease to a distant point of the port. Its proximate growth and a continuous current are marked in its course and stages by human victims.

I quite concur with Mr. Gamgee that currents of pure air, driven through and through infected places, constitute the most reliable means to dilute and ultimately to destroy the poison. The only danger is that atmospheric dilution, up to the present time, has implied a city's doom—a wholesale infection. In grappling with the great difficulties which present themselves, as a believer in the oceanic character of yellow fever, I must first and foremost attend to ship sanitation. Medical men are not engineers and ship-builders. Had they known how to advise, the practices commonly witnessed in gulf ports would long since have ceased.

The problems relating to ship ventilation and pumping the bilge water are of the highest importance; but I could not treat these subjects technically, and I desire to restrict my observations to a few reforms and procedures which admit of prompt attention.

So far as the ship is concerned, it should not be emptied of solid cargo until it is freely exhausted of its gaseous contents. All that is then removed from the ship should be freely aerated by positive currents of air, and in many cases there is no objection to sulphurous acid fumigation.

The empty ship can then receive in its bilge some fresh burnt lime, and this may be sprinkled with advantage where foul gases may emanate. The word cleanliness imputes, then, all that has to be secured; and by cleanliness and free ventilation, with thorough liming, a vessel can be purified. If, however, a low temperature can be secured, any way below 32° Fahr., and made to penetrate somewhat, the poison can certainly be destroyed.

Mr. Gamgee has suggested to me a means whereby the foulest timber can be purified by hot drying oils, which destroy all moist organic matter, and render the wood ever after impervious to moisture or elements of decay. The hot varnish could be applied to the whole interior of the ship with great advantage. An impermeable purifying coating may prove a great boon, and this constitutes one of the points which Professor Gamgee intends to carry out in his fever-proof ship.

The great advantage of having ships carrying skilled men and appliances for disinfection, is that infected vessels need never touch land until they are made safe. It is a bright idea, and could undoubtedly be carried out into practice in our gulf ports.

Stations, such as the one at Ship Island, may be adopted, but we are threatened, with that great impediment to common sense legislation in sanitary matters, by divided counsels within states, and inadequate national administration. A sanistation could easily be provided for Havana by the Cuban government, and seeing how effectually the Spaniards have excluded yellow fever from their European Atlantic ports, it is to be hoped they may disregard the recently uttered statements, that Cuba is to be the permanent hot bed of a naturalized pestilence.

I shall not attempt much, on this occasion, in relation to prevention of yellow fever on land, but I am happy to have been the first to witness Mr. Gamgee's experiments, whereby

forced ventilation of rooms, with filtration and effectual disinfecting of air is rendered possible. He has designed an apparatus which he calls the "circlair," which enables us to shut up a room and practically isolate the volume of infected air, which is abundantly purified and displaced by fresh air.

The poison which seems to travel on the roads and sidewalks, with the volume of heavy air dropping from infected houses, can, in my opinion, be better destroyed by hot lime than carbolic acid. I object to this offensive compound, and believe, that irritating and fetid vapors have only found favor, because people have been led to suppose they must do good, and that their presence was unmistakable.

I do not think the yellow fever poison is difficult to destroy, but we must not be idle when a case indicates its location. Simple means will effect much, and I am in favor of prime attention to the foul gases of ships, the foul air of cities, and notably of the sick room. Control these, and much has been done to control the malady.

## THE MOST RECENT UTTERANCES ON ACCLIMATIZATION AND ENDEMICITY

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### I.

#### ON ACCLIMATIZATION.

The 1880, August number of the *New Orleans Medical and Surgical Journal* contains an excellent article by Dr. Chaillé, on acclimatization, or acquisition of immunity, from yellow-fever.

Having ventured to take exception to the preliminary report of the Havana Yellow-Fever Commission, and especially to the lack of all, in the nature of true evidence, adduced, in opposition to the nautical origin of yellow-fever, I am all the more anxious to give my hearty approval to an entirely new departure, on the part of Dr. Chaillé. It will be seen that I have spoken of immunity from yellow-fever being alone obtained by an attack—mild or severe—of the disease.

Dr. Chaillé, while in Cuba, gathered from eminent physicians a number of yellow-fever cases in Cubans—not only born—but always residing in Cuba. A very intelligent minority of the medical men of the Island regard immunity as only acquired by an attack of the disease.

Heinemann reports, in 1879, “until lately the physicians and people of Vera Cruz, supported, with fanaticism, the dogma that natives were absolutely exempt from yellow-fever. But the fearful epidemics of recent years (1875, 1877, 1878,) have worked a change; for so many native children and adults suffered that the truth could no longer be denied that they do not enjoy an absolute immunity.”

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\*From the Louisville Medical Herald, September, 1880.



By far the most interesting and conclusive evidence is afforded by Dr. Chaillé's observations in relation to New Orleans. He says: "In 1851, the medical profession of New Orleans was almost unanimous in teaching that those born in that city were not liable to yellow-fever. Prof. Warren Stone, my teacher, was one of the very few who taught that Creole children did have yellow-fever, often in a form too mild to justify an absolute diagnosis."

Dr. Chaillé has lived to find that Prof. Stone's view has triumphed, and that the change has been so complete, he attributes "chiefly to the fact that, until 1858, New Orleans was ravaged by almost biennial epidemics, while since 1858, there have been only two serious invasions, in 1867 and in 1878. The longer the intervals between epidemics, the larger necessarily must be the number of those who have failed to acquire immunity, and the more glaring becomes their liability to the disease. This is the explanation of the very old observation, that, the longer the absence of an epidemic from a place, the greater the susceptibility of its inhabitants.

"Wherever yellow-fever occurs only occasionally, as an epidemic, there the Creoles are manifestly liable to the disease; wherever it prevails habitually, there the Creoles appear to enjoy a very great, if not absolute, immunity; and the fact that the white natives of a place do enjoy this comparative immunity, is good evidence that the poison of yellow-fever prevails habitually in this place. Hence, the important practical conclusion that the stranger should beware, during the warm season, of every place in Cuba or elsewhere, of which the natives boast, that while the foreign born habitually, they never suffer with yellow-fever."

Neglecting much instructive matter, for which the reader can refer to the original article, it will be seen that Dr. Chaillé endorses my statement at page 28, in the follow-

ing words: "The only known mode of acquiring immunity from every other non-recurring disease is to have one attack; and, so far as yellow-fever is concerned, while various modes are claimed this remains the only one so certain that no one whatever disputes it." Susceptibility to the poison is sometimes unaccountably diminished. He says: "facts will now be presented in proof that immunity from yellow-fever is acquired by a large majority of the natives of infected places, in the same manner it is acquired by unacclimated immigrants, and in the very same manner that immunity from every other non-recurring disease is acquired."

Dr. Chaillé appends a table, consisting exclusively of official data of population, from the United States census, and of deaths from the Board of Health of New Orleans. A second table consists of results obtained by calculations based on the original data in the first table—calculations designed to facilitate the interpretation of those original data. They date back to 1856—that is as far back as authentic records can now be procured, including those most disastrous epidemics—1858, 1867 and 1878.

Until 1858 the great majority of the medical profession of New Orleans taught, with as much vehemence, as some few of them still do, and as nearly all the physicians of Havana and Cuba now teach, that the native-born Creole children and adults enjoyed absolute immunity from yellow-fever.

Dr. Chaillé has classed his conclusions under six heads:

*First.* There is an enormous excess in the deaths of children, under ten years of age, during every epidemic year; and this mortality occurs during the very months when yellow-fever devastates the city.

*Second.* The statistics turn aside from the simple and reliable tally of the sexton, and become dependent on the diagnostic skill and theories of the doctor, since simul-

taneously with yellow-fever there is always an enormous increase in deaths from "bilious," "congestive," "pernicious," malignant," fever, as well as by typhus and typhoid. Not very flattering to the doctors who have hitherto failed to perceive they were recording a tale of singular inconsistency and incompetency, which "the sexton's tally" in Dr. Chaillé's hands has just exposed.

*Third.* Yellow-fever has not only been confounded with "malarial fever," since all these deaths, added to those of yellow-fever, fail to sum up a total, equal to the excess of deaths, which invariably occurs during epidemic years. Faulty diagnoses seem to include convulsions, cerebral congestions, and that everlasting ghost of mothers—"teething." With such a confession or exposure of prejudice or downright stupidity, who can be astonished that the average medical man knows nothing of the origin and nature of yellow-fever.

*Fourth.* Accepting the excess of children's deaths, in epidemic years, as due to yellow-fever, the comparative susceptibility at different ages, can be calculated. There was an increased mortality, but slight, in children under one year. It was much greater between 1 and 2 years. Enormous in children from 2 to 5 years old. So that there were 89.4 deaths in every 1000 of those in 1878, in place of 19.6 deaths in every 1000 in the non-epidemic years of 1877 and 1879. This, more than quadruple increase, is not equalled at any other ages. Dr. Chaillé grapples with an anomaly exhibited by the epidemic of 1858.

He says: "When it is known that the population of New Orleans had been subjected, so shortly before, to the three violent epidemics of 1853-'4-'5, and that during this prosperous period of the city's history, unacclimated immigrants, chiefly from 15 to 40 years old, flowed to it in numbers far greater than subsequently, no one will be surprised to find that the mortality in 1858, compared with



1878, was less in those under 15 years, and greater in those from 15 to 40 years of age."

*Fifth.* "Accepting the conclusion, drawn from the cemeteries, that the excess of deaths during an epidemic are due to the disease causing the epidemic; and, again, taking the 1878 epidemic as an illustration, we are forced to admit that somewhere between 1406 and 2023 children, under 10 years of age, must have been killed by yellow-fever." Dr. Chaillé attempts to deduce the number of children protected, having recovered, and these could not be less than 6,000; but probably much more, since, the younger the children, the less fatality amongst them from this disease.

*Finally.* Dr. Chaillé wisely refers to mild, benign, aborted or bastard yellow-fever, granting immunity, and which is recognized by physicians of noted skill.

I cannot resist quoting a paragraph, which affords me an opportunity of directing attention to the opinions of a very distinguished British physician. Dr. Chaillé says: "Blair, the unsurpassed classical student and historian of the epidemic of British Guiana, 1851'-4, states that the very first cases occurred among little children, and that the attacks of others were numerous and repeated. Infancy was one of the most favoring causes of the action of yellow-fever poison. The constitution of the new born or young white Creole was highly susceptible. He or she was truly in the category of new comers. In 1851 yellow-fever had been absent from British Guiana for six years—since 1845."

Dr. Daniel Blair, thus justly eulogized by Dr. Chaillé, was Surgeon-General of British Guiana in 1852, when his work reached a third edition. He states that, when the epidemic of 1837 occurred, those who had seen the disease in 1819, had retired from practice, or were dead, and had left no record of its local peculiarities and treatment. When, in 1852, he attempted to trace the disease to its source he said,



"sufficient evidence has been elicited from the last epidemic to show that our inquiries must be directed to the sea-shore." And unable to perceive, in common with many others, that ships had infected the country, his perception was, nevertheless, quick enough to make him say "some new element is required in the generation of yellow-fever, beside what is to be found annually within our embankments; and it is in all probability dependent on a sea change."

## II.

## ON ENDEMICITY.

Dr. S. S. Herrick, one of the editors of the *New Orleans Medical and Surgical Journal*, in a review at page 142 of the August number, says: "We do not regard the term "endemic" as properly applicable to yellow-fever in New Orleans." \* \* \* "An endemic should prevail in its habitat every year; not so with yellow-fever at New Orleans." Further on he remarks that, the doctrine of yellow-fever endemicity at New Orleans, besides being scientifically unsound, is ruinous to the city's commercial prosperity..

"The suicidal policy of a portion of our citizens in proclaiming to the world that their soil is incurably plague-stricken, *has no parallel in this country, nor probably in the whole world.*" Dr. Herrick has evidently not read the yellow-fever literature of his country, if he believes the words we have italicized. The indignant remonstrance of the inspector of vessels of Philadelphia in 1793, Dr. J. Hutchinson—who denied that the fever was "an imported disease, is before us. Dr. Rush, we well know concurred, and Caldwell echoed their sentiments.

As late as 1819 we find Dr. Samuel Emlen producing a statement relating to Philadelphia, "as additional confirmation of the disease occurring without the slightest foundation for tracing its origin to a foreign source."

Dr. Thomas Lawson, Surgeon-General, in his statistical report, during the sickness and mortality in the Army of the United States, in 1840, said: "The experience of several centuries teaches us, that the cause of this fever is perennially present in our Southern cities. Indissolubly connected with our soil and climate, it maintains the same relation towards the animal system as the malaria of our Southern low lands."

I allude, casually, to one or two references, which indicate that the people of America have, ever since yellow-fever invaded our ports, blamed their soil for it—so much so that the eloquent words of Dr. Francis before the Third National Quarantine and Sanitary Convention, held in New York in 1859, never can be forgotten; there he vindicated the City of New York from the imputation of being the birthplace of yellow-fever. He spoke of it as engendered on ship-board, in healthy vessels sailing from ports diseased of yellow-fever. He complained loudly of those citizens of America who affixed to this noble country the stigma of producing yellow-fever. Nevertheless scores of partial observers, ignoring the past, the experiences of which can only be made available by industry and patient investigation, if the truth be sought, have steadfastly adhered to the local origin of yellow-fever in our ports, and the last stronghold of this baseless and infamous theory is New Orleans!

Can we be astonished if Griesinger, in his treatise on infectious maladies, published in 1868, should say "yellow-fever develops under the influence of general climacteric conditions? It is essentially a disease of the Western Hemisphere, of the West Indies, and of the *American Continent*."

The mere quotation of this sentence suffices, I hope, to condemn the unfounded assertions, which, to this day, pass current as worthy of report.

Dr. S. S. Herrick must know that, now the evidence relating to the nautical origin of yellow-fever is, in a substantial measure, before the world, and no one has dared to produce that which does not exist, *counter evidence*, to dispute it. His clear endorsement of my views as to the endemicity of yellow-fever, I acknowledge, with thanks. He asks some questions which familiarity with the yellow-fever history would have indicated as entirely irrelevant.

It is, perhaps, only fair to my case, and certainly fair to Dr. Herrick, to quote verbatim the extraordinary questions he has proposed.

He says: "If the fever springs from the tropical seas of America, why does it not become diffused throughout the warm waters of the Atlantic basin, at least, and thus infect all its shores? Having reached the western extremity of the Mediterranean Sea, and the coast of Spain, why should it not have extended eastward its whole length? Why is it not an every day occurrence for vessels, sailing between Europe and New Orleans, to catch the yellow-fever in the infected Gulf of Mexico? Why does yellow-fever prevail annually ashore at Vera Cruz, and never on the shipping, which has to be a mile distant from shore? Why are vessels lying in the harbor of Havana, which discharges and receives cargoes by lighters, and whose crews are not allowed to go ashore, so much less likely to become infected than those which lie at the wharves? Until the above inquiries are answered satisfactorily, we ask to be excused from accepting any *dropsical* theory of the origin of yellow fever."

They have been answered repeatedly by the events of past times. The numerous and cogent reasons which explain why certain animals, like the herring, do not increase in numbers to the exclusion of all others, may be cited in relation to an universal law of limitation of development. Who ever suggested that the Gulf of Mexico was itself infected, or that the Atlantic ocean waters themselves, under the benign influence of air, sunlight, and electricity, could produce yellow fever? Dr. Herrick has erected a house of cards to experience the gratification of its demolition. The sailors on such ships as the *Excelsior*, so long as they keep above deck, are usually exempt of the products of foulness which arise from the *imprisonment* of Atlantic Ocean-



*water* and *products* in a ship's bilge, or in rotten timbers. When, in 1840, the General Board of Health met in London to report on quarantine, what did they say: "A foul ship is not only a centre of disease to those on board, but a source of disease to her neighborhood." Whilst condemning the quarantine establishments, the report states "that typhus and other dangerous epidemic diseases are frequent on board merchant seamen vessels at sea and in port, for which no effectual or suitable provision is at present made." Dr. Herrick allies yellow fever with typhoid—the typical land endemic whose periodical development, *de novo*, in our mountain ranges and elsewhere, is almost demonstrated. The analogy between yellow fever and *typhus* is universally acknowledged, whilst recognizing some marked and unfailing points of demarcation between them. Surely typhus may be regarded as a specific disease, due to a specific poison in foul, over-crowded habitations, but no modern pathologist can ally typhus or yellow fever with small-pox, the type of the purely contagious diseases. It is its place in nature, which has been, for the first time, defined by the believers in the *naval* origin of the yellow fever, in the tropical Atlantic, and this has especially, and for the first time, been accomplished by my friend, Mr. Gamgee. It is satisfactory to have lifted Dr. Herrick so far on the fence, as to make him admit that yellow fever is not an endemic disease, "*that is* attributable to localized causes in any region whatever." As regards land, he is quite correct, and as regards seas, he is technically right—for remove the foul, and often becalmed shipping from the tropical Atlantic ocean, and yellow fever will be found without a habitation, and, for all practical purposes, not needing a name. It is by discussions and experiment that the strength of our position will be forced on the medical profession, and all we ask is a fair field and ample opportunity to prove our point or be enlightened by the correct interpretations of accumu-



lated experience, which no special pleading can now displace from the category of reliable human records. Yellow fever history counts some miserable exhibitions of human folly and prejudice, which retarded progress for years, and none more so than the conflict between Bancroft and Chisholm, in which the latter proved triumphant. In this controversy the point was as to the actual importation of the disease into the West Indian Islands, and that most honest, most impartial and pains-taking American physician, Dr. John W. Francis, stated before the 1859 Convention, that "no fact in medical history is better established, in the whole range of historical testimony concerning endemics, notwithstanding the obloquy attempted to be cast on Chisholm by the late Dr. Caldwell and others, and by Bancroft, an equally unscrupulous writer."

Dr. Herrick's reference to the ship origin of the yellow fever in the Atlantic "as a *dropsical* theory of yellow fever" is interesting to me because, hereafter, when the question of priority turns up, in relation to the correct and only possible interpretation of the origin of yellow fever, we shall be able to point to the ridicule so commonly heaped on the promulgators of new truths and of the most important discoveries.

If Dr. Herrick has any facts to furnish we shall accept them with as much thankfulness as I now express to Dr. Chaillé for his lucid arguments and convincing statistics, relating to the subject of acclimatization. The Northampton case I shall hereafter discuss. The weapons of philosophy are *ascertained truths*, and the purely empirical method pursued by many of the ablest medical observers of yellow fever in the past, has crushed out the theory of the land origin of the disease—demolished the doctrine of a specific animal virus, like that of small-pox, and traced epidemics of yellow fever to putrefaction in foul ships sailing, or that have sailed, on or near the Atlantic Equator. Directly or indirectly, this is the essential source.

4

## THE ORIGIN OF THE POISON OF YELLOW FEVER.

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Having both approved and refuted opinions and statements, hitherto made by Doctor Chaillé, it is opportune that I should comment on that chapter, of the final report of the Havana Yellow Fever Commission of 1879, which has just made its appearance, in the October number of the New Orleans *Medical and Surgical Journal*. My greatest difficulty, in accomplishing this, is to give an adequate reflex of Doctor Chaillé's attempt, since he has laid his case before the medical world, without condescending to analyze the vast array of accumulated evidence.

We are entirely in accord with our adversary on the results of biological investigation. Spontaneous generation of organic forms, or of the specific virus of diseases propagated alone by contagion, cannot be admitted. The brief reference to fungi is entirely unobjectionable, except as bearing in any sense on the untraced lineaments of the yellow fever poison. The germ theory has been made to do service, after it has been proved as calculated to mislead the world. The systematic labors of the Calcutta Sanitary Commission, in relation to Hallier's cholera fungus, are in striking contrast to the attempts of the Havana Commission to fasten on us the "*unknown*."

Pasteur's researches are justly eulogized, but none are in conflict with the opinion, founded on hundreds of carefully recorded cases, that a putrefying bilge-water, proceeding from a definite region and under specific conditions, emits poisonous elements, destructive to human life and susceptible of increase outside the human body. There is one feature of the deadly bilge which must again be forced on public at-

tention. It is the exclusion of oxygen, and the continued development of foul emanations almost in proportion to such oxygen exclusion. Every student of biology must recognize that this implies some physical or chemical transformation—a death change, in the imprisoned living water of the equatorial Atlantic, when charging the hold with pestiferous miasm. Pasteur's results are incontrovertible, and indicate the pre-eminent mastery of natural law. They add to all previous demonstrations that "like reproduces like," but they would involve us in a confusion worse confounded, if they implied, and they do not, that we must class erysipelas or purulent infection with syphilis and variola.

One typical characteristic of yellow fever poison is its relation to temperature. A change of physical state is absolutely essential to its production and destruction. Being a tropical disease, its continuance in a semi-tropical climate depends on protection from climatic influences in ships, houses, boxes, &c. How can a poison carried anywhere on a pin's point be classified with one, peculiar to fomites, always needing encasement for distant fruition and transportation?

We might have expected a new and faithful picture of this intangible yellow fever poison, culled from Cuban experience and genuine scientific investigation, instead of vague analogies, and the most superficial reference to authors and observers, who avowedly have seen or written on other diseases, knowing nothing of yellow fever. Doctor Chaillé remarks that outbreaks of yellow fever on vessels "for the most part are not reported with sufficient precision respecting either dates or other essential details." This is not so; and if so, why has he neglected those of the minor part? For one case, in which there is any indication of direct dependence of an outbreak on the intra-

corporeal development of the yellow fever poison, and the direct transmission from the sick to the healthy, as in the case of scarlatina or small pox, there are thousands demonstrating that these exceptions are entirely illusory. There is absolutely no reliable evidence, on the inevitable dependence of cases of yellow fever on pre-existing cases. There is an overwhelming amount indicating that the emanations from a ship's hold have killed crews and desolated sea ports. Dr. Chaillé has not read, or, if he has read, he has suppressed the only evidence in existence. I cannot do better than conclude by two letters from Mr. Gamgee's pen :

#### YELLOW FEVER POISON.

DEAR DR. HARGIS: The battle is won. It was quite impossible for Dr. Chaillé to admit the land origin of yellow fever, and, having condemned, as erroneous, the view that it is the ship fever of the Tropical Atlantic, he was compelled to class it, as we anticipated, with the pure contagia. Few can be misled by his presumed analogies and pure hypotheses. Those who read history will find the evidence of ocean origin so strong that they cannot set it aside. The competent medical practitioners, who know yellow fever as it occurs in ships and sea ports, will be unanimous in admitting, that the human body is not a necessary factor, in the production and transmission of the poison. Imprisoned air is its vehicle. When concentrated it manifests instant effects. If weakened by dilution a supposed incubation with no definite limits, but always short, conforms to the action of ordinary poisons.

Had our attempts, at setting fairly before the world the evidence and arguments in favor of the naval origin of yellow fever, not met with counter statements—*for of counter evidence there is none*—it might have taken years to instil the truth in the public mind. Dr. Chaillé has rendered the cause great service, and by the time we can adequately ex-



pound the entire question, numerous independent students and reasoners will confirm our teaching. I am seeking in every direction for data against us, since it is my invariable practice to avoid, by every means at my command, any *partial* acquisition of truth which, in its blinding influence, is most detrimental.

You will perceive from the annexed letter, to Rear-Admiral Daniel Ammen, United States Navy, that I have advanced in my inquiries, as to the localization and development of yellow fever, before it ever acquired the precarious foothold in the West Indian and American sea ports, which, for over a century, has misled medical observers as to the home of the disease.

If the Spanish domination of Cuba, with its incessant turmoils, ceased to-day, Havana would again merit Catesby's eulogium. In his Natural History of Carolina, Florida, and the Bahama Islands, published in 1743, Mark Catesby, Fellow of the Royal Society, says:

"The north side of Cuba also enjoys the benefit of the refreshing winds, particularly that part of the island on which the Havana stands. To this, no doubt, is owing the healthiness of the air and good character of that proud emporium, the conquest of which, by British arms, would put us in possession of a country much more agreeable to British constitutions than any of the islands between the Tropics."

I remain, dear Dr. Hargis,  
very sincerely yours,

JOHN GAMGEE.

WASHINGTON, *October 21, 1880.*

# LETTER TO REAR-ADMIRAL AMMEN, U. S. N.,

ON

THE INTER-OCEANIC SHIP CANAL

AND THE

YELLOW FEVER ZONE.

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SIR: Captain De Kraft's interesting note on the respective merits, in a nautical point of view, of the Panama and Nicaragua sea approaches, suggests a statement of the relation the equatorial calms bear to yellow fever development.

Brief reference to the results of historical research is essential to an understanding of the question.

Prior to the voyage of Columbus,—and, indeed, for long before his birth, the records of plagues had acquired clearness and precision. No doubt preoccupies the medical historian as to the consequences of the Crusades. The source and periodic invasions, in Central and Western Europe, of the Black Death were traced by keenest intellects. Lazzaretti and quarantines had been established. The origin of scurvy and its manifestations, in the days of Venetian supremacy, down to Vasco de Gama's voyage to Melinde in 1498-99, have been handed down to us in interesting narratives. Columbus crossed the ocean in shallow, small and, in the case of two out of three, deckless vessels. No sign, no trace, no word of yellow fever, till after his first voyage. The native West Indians had no ships and no seaports—ergo, they had no yellow fever. The followers of Columbus first perished from it, navigating the Atlantic calms, and some, perhaps many, Indians caught it in the new shore settlements.

A wide stretch of the imagination has accounted for the depopulation of the islands by yellow fever. Ancient habits were crushed out by sword and torch. Fields re-

mained unsown. Famine fever spared neither hill nor dale. It penetrated where yellow fever, as a pestilence, never could prevail. Since then, the islands of the West Indies and likewise this continent afford no trace of yellow fever epidemics, except as seaports grew and sailors multiplied. Voyages of discovery, Slavers from Africa plying their vile trade since the days of Columbus, Pirates from the North Seas, bore human beings to and through the Torrid Zone, and yellow fever became constant in ships. It was spoken of as "the effects of climate," flux, malignant fever and more specifically "calenture." The Portuguese entitled it "*morte repentina*."

The first Frenchman who visited the East Indies, Francois Pyrard, relating his voyage of 1601, gives a graphic description of the dangers incurred from "*injuries de l'air*" on reaching within seven or eight degrees of the line and during the passage of the equatorial calm. The uncertainties of navigation until the determinations of longitude, which practically began with Major Holmes' pendulum watches at sea in 1665, led to serious detentions. Dr. Stubbs stated, in the Philosophical Transactions for 1668, that "the change of climate and the effect of it are sensible to our bodies as we approach the tropick. There usually occurred, as you may observe in Purchas' voyages, sicknesses in our ships about that time; and as soon as the seamen pass the tropick they still use expressions of joy by firing guns in testimony of gladness for their safe arrival.

\* \* \* \* \* In our ships two had the disease so much talked of called the calenture."

Queen Elizabeth, whose proudest title was that of "Mistress of the Sea," granted a charter to the East India Company in 1600. The first fleet that left lost a third or more of its people in the tropics. The calenture appeared as they approached the line. It passed away as it came, without warning, when the vessels reached south of the calm

belt of Capricorn. East Indian commerce confined yellow fever, mainly, to the east of  $30^{\circ}$  W. Longitude. The ships left Europe sound, became infected after passing the Tropic of Cancer, and usually in the equatorial calms. The malignancy of the fever disappeared near the Cape of Good Hope, and scurvy commonly succeeded it in the Indian Ocean. This phase of plague history has remained unnoticed, and my attention was specially directed to it by the well known outbreak of yellow fever on the Busbridge East Indiaman in 1792, which I felt sure could be no isolated instance, but must have conformed to a general law.\* That the calenture of the equator was yellow fever, commonly affecting European traders with the East Indies, all history confirms. Ships returning from the Indian Ocean, China seas or Australia, suffered inconvenience only when reaching north of the calm belt of Capricorn, but especially north of  $5^{\circ}$  S. latitude. The disease disappeared north of the Tropic of Cancer.

To the epidemiologist no more interesting subject presents itself, than tracing the early beginnings, the greater developments, and the present decline of yellow fever. Where mystery and chaos reigned before, we find order now.

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\*Alexander Stewart, surgeon to the Earl Talbot and General Goddard, East Indiamen, writing in 1798, said, "It is a well-known and melancholy truth, that these voyages when protracted beyond the usual length of time, from one or other reasons have hitherto been very generally attended with great sickness and mortality; the scurvy, fluxes and malignant fevers, having frequently made dreadful ravages amongst the crews" \* \* \* \* \* "The foul air that is generating in the well, which is known to be of a very noxious and fatal nature, should be frequently purified by fire."

"When the bilge water is much corrupted it should not be pumped out before breakfast, as in the morning, with an empty stomach, men in every situation are more liable to be affected with any noxious effluvia."

"It may be alleged that sickness and mortality in voyages to India are now less frequent than in more early times. It will be found, however, that this results not from a more particular attention to the preservation of health, but is chiefly attributed to the great improvements in navigation, which now effects a passage in a quarter or third of the time usual in former periods."



Bearing in mind the statement just made, it is clear how French ships infected Barbadoes in 1647. In 1671 the victorious fleet from the Panama expedition introduced the pestilential fever into Jamaica. In the year 1690 a bloody revolution took place in the Empire of Siam. The French, established there, embarked for France in the ship *Ori flamme*. They were forced by stress of weather into Fort Royal, Martinique, communicating a contagion named the "Mal de Siam," which not only assailed the Port, but all the vessels in the harbor. No one need be told to-day that Siam is beyond the yellow fever zone, nor that the *Ori flamme* succumbed to the disease in crossing the tropics. She affords an early instance of the practice condemned by Humboldt. "In all climates," he says, "men imagine they find some consolation in the idea that a disease reputed pestilential is of foreign origin. As malignant fevers are easily engendered amid a large crew crowded together (entassés) in filthy vessels, the commencement of an epidemic dates pretty often from the arrival of a squadron."

How slowly American sea-ports became occasional centres of devastation I need scarcely recount. Boston, *not a Southern city*, first imported the disease, in 1693. Six years later Philadelphia was attacked, and in 1702 New York was, for the first time, invaded. Before the Revolutionary War there was little commerce, and whilst the pure contagia—typically belonging to no distinct region, no altitude, no latitude nor longitude, such as small-pox—ravaged the country, the so-called West Indian fever had but slender chance for more than very limited introduction in the cities of the shore, and never reached inland. The Revolutionary War effectually excluded it. The Siam fever was forgotten, but ports in Spain and France were contaminated, owing to their trade with the West Indies. These were, however, free at intervals, until a new era was reached, when the ship *Hankey*, necessarily crossing the line,

arrived from Bulam, early in the year 1793, and started the most virulent plague epoch which ever assailed this country. The massacre of San Domingo added to all its horrors the cruel slaughter, by yellow fever, of the hospitable succorers of the Creole refugees, who were fortunate enough to reach American ports alive.

The College of Physicians of Philadelphia, whose 1797 Memorial, to the Senate and House of Representatives of the Commonwealth of Pennsylvania, saved the honor of medicine at this period, effectually disposed of the opinions of Rush, Caldwell, and other professional advocates of the indigenous origin of the disease. That memorial rebutted the monstrous proposition that yellow fever was a plague of the American Continent. It traced it "to the infected clothing of persons who died in the West Indies. In most of the cases where the infection cannot be ascertained, the first appearance of the disease has been, as in the other instances, in the neighborhood of the shipping, or among persons connected with vessels." . . . "In most of the instances of the occurrence of the disease in the United States, there has been war in the West Indies." Referring to these wars, Burke said: "In these adventures, it is not an enemy we have to vanquish, but a cemetery to acquire." Our gallant British troops, destined to encounter barbarians, and "most of them in the bloom of youth, were conveyed," says Bryan Edwards, "with little intermission, from the ship to the hospital, from the hospital to the grave!" Where every advantage was "but a new demand for recruits to the West Indian" tombs, entire regiments being exterminated, how could America escape the hideous consequences of unwholesome intercourse?

Napoleon repeated the deluge of human blood, sacrificed to yellow fever, by his memorable West Indian expedition in 1805. The contest raging in Europe, and the Berlin decree of 1806 blockading the British Islands, interrupted the

profit America was deriving from the destruction of the ships and commerce of other nations. The President recommended to Congress that the seamen, ships, and merchandise of the United States be detained in Port. The embargo was established, and until its removal by Madison on his accession to power in 1809, ocean commerce and yellow fever were alike extinguished. The plague returned in 1811, but the war of 1812-14 again closed the Atlantic sea-ports, and complete immunity reigned till commerce began to revive in 1817. A new factor soon came into operation in the shape of emigration to the Southern States, and imperfect quarantine admitted the disease annually, not only into the ocean harbors, but up the rivers accessible to the West Indian ships. The most active cause of virulent outbreaks in the Islands was the active, irregular, and barbarous traffic in slaves.

The events of the late rebellion, sustaining previous history, are too fresh in the people's minds to need exposition here. I need not unduly extend this cursory glance at the invasions of the only genuine ocean pestilence on the Globe. The Equatorial calm belt continued the centre of uncertain and usually prolonged detentions. Steamships then opened a new era of yellow fever decline, and American hydrographers contributed most, by their records and charts, to reduce the passage of sailing ships from the North to the South Seas. To the latter circumstance must we ascribe the comparative immunity enjoyed by Australian clippers, which have, however, at times encountered the ship fever of the tropical Atlantic—often escaping with less mortality than the old East Indiamen. Had the medical profession of New Orleans followed their great teacher, Carpenter, they might long since have seen that whilst the Mexican Gulf, like all other almost land-locked seas, is free from transmissible ship-fever, the annual and, frequently, disastrous importations of the disease by vessels from Eu-

rope, was due to the tedious course compelling detention in the calm belts. The passage through the Caribbean Sea is the occasion of its manifestation, and on the northward progress to New Orleans the disease acquires epidemic proportions. On leaving the Gulf for Europe a vessel at once enters the Gulf Stream and the trades, and is soon blown beyond harm's reach. Its purification, on the speedier eastward voyage, is due to similar influences to those which operate South of Capricorn in ships bound for the Indian Ocean.

To conclude :—the hottest open sea in the World, the western half of the Equatorial Atlantic, is the constant centre of yellow fever development in becalmed ships. The eastern half once was, owing to the peculiar conditions of British East India commerce, the perennial seat of malignant naval outbreaks, always commencing and ending in mid-ocean. The opening of the proposed Panama Canal, level with the sea, in the very midst of the Equatorial Calms, would, in my opinion, have the following effects, until sailing ships and wooden vessels, liable to decay and contamination, are abolished.

In the first place, the long voyage from Europe would often result in sickness by the time a vessel reached the extreme west of the tropical Atlantic. Slow towage through the canal would not help it. Its abandonment to six hundred miles of shifting winds and calms in the Pacific would seriously aggravate its ills, and might tend to unprecedented extensions of the disease in that Ocean.

Secondly. The peculiar properties of Ocean water with its teaming forms of life, which undoubtedly account for the poisonous bilge-emanations of vessels in the equatorial Atlantic, would invade the canal water and probably extend into the Pacific. An argument against this theory, however, may be that the colder waters of the Pacific might oppose this westward current. It must not be forgotten that



the strip of land between the oceans effectually separates their waters, fauna and flora. Antarctic ice maintains the high average salubrity and immunity from yellow-fever of the equatorial Pacific, in contrast to the opposite conditions in the equatorial Atlantic.

Thirdly. Vessels reaching the mouth of the Nicaragua Canal more speedily, and blown across the fresh water lake by the constant breezes which disturb it, would be placed in the best possible sanitary state, by free ventilation as in the Ocean trade winds and by washing out of any putrescent ocean residue. They would pass into the north Pacific current free from the dangers incurred in and beyond Panama Bay.

I have not entered on the dangerous land fever which assailed the laborers on the Panama Railroad, since my field of inquiry has been more especially in relation to that equatorial plague, which has been limited in its Pacific extensions by the purifying influences of wind and cold, in the passage round the Horn.

The localization of yellow fever and the limitations by natural conditions, so universally observed during the past four centuries, point to the facility with which this continent may be permanently and unfailingly protected from that disease. All ships from the yellow fever zone, rather than those only from infected ports, should, during the dangerous months, be purified before contact with land. This demands machinery and disciplined crews afloat, adequate to ship purification in any latitude.

Contact with quarantine grounds leads to outbreaks, such as have caused dangerous medical dissensions in the recent outbreak on the Mississippi. Medical disputants continue their doctrinal defences, whilst the Mississippi Valley lies exposed to the utmost danger. Frost ends the discussion by disappearance of the disease. There is but one method whereby this country can be permanently protected from the scourge. Cold is its only known and

available antidote. Fumigations and fetid disinfectants have all signally failed. The construction of the Refrigerating Ship, approved by Congress, would afford a demonstration surpassing in importance all else attempted in relation to yellow fever prevention.

Addressing these words to an Admiral of the United States Navy, I feel confident they will not be deemed out of place.

I remain, Sir, with much respect,

Your obedient servant,

JOHN GAMGEE.

RIGGS HOUSE, WASHINGTON, D. C.,

*October 18, 1880*

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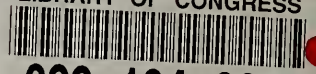








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